PROCEEDINGS

528-81

OF THE

ROYAL SOCIETY OF LONDON.

From May 2, 1889, to November 30, 1889.

VOL. XLVI.

LONDON:
HARRISON AND SONS, ST. MARTIN'S LANE,
Printers in Ordinary to Her Majesty.
MDCCCXC.

LONDON:
HARRISON AND SONS, PRINTERS IN ORDINARY TO HER MAJESTY,
ST. MARTIN'S LANE.

CONTENTS.

VOL. XLVI.

No. 280.—*May* 2, 1889.

May 9, 1889.

May 16, 1889.

No. 281.—*May* 23, 1889.

No. 282.—*June* 6, 1889.

No. 283.—*June* 20, 1889

No. 284.—*November* 21, 1889.

November 30, 1889.

ANNIVERSARY MEETING.

PROCEEDINGS OF

THE ROYAL SOCIETY.

VOL. XLVI. No. 280.

CONTENTS.

May 2, 1889.

May 9, 1889.

For continuation of Contents see 2nd page of Wrapper.

Price Six Shillings and Sixpence.

CONTENTS (*continued*).

May 16, 1889.

HARRISON AND SONS, 45 & 46, ST. MARTIN'S LANE, W.C.,

AND ALL BOOKSELLERS.

PROCEEDINGS

OF

THE ROYAL SOCIETY.

May 2, 1889.

Professor G. G. STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

In pursuance of the Statutes the names of the Candidates recommended for election into the Society were read from the Chair as follows:—

Aitken, John.
Ballard, Edward, M.D.
Basset, Alfred Barnard, M.A.
Brown, Horace T., F.C.S.
Clark, Latimer, C.E.
Cunningham, Professor David Douglas, M.B.
Fletcher, Lazarus, M.A.
Hemsley, William Botting, A.L.S.
Hudson, Charles Thomas, M.A.
Hughes, Professor Thomas McKenny, M.A.
Poulton, Edward B., M.A.
Sollas, Professor William Johnson, D.Sc.
Todd, Charles, M.A.
Tomlinson, Herbert, B.A.
Yeo, Professor Gerald F., M.D.

Professor Georg Friedrich Julius Arthur Auwers, Foreign Member (elected 1879), signed the obligation in the Charter Book and was admitted into the Society.

The following Papers were read:—

I. "Note on the Effect produced by Conductors in the Neighbourhood of a Wire on the Rate of Propagation of Electrical Disturbances along it, with a Determination of this Rate." By J. J. THOMSON, M.A., F.R.S., Cavendish Professor of Experimental Physics, Cambridge. Received April 1, 1889.

In a paper on "The Resistance of Electrolytes to the Passage of very rapidly Alternating Currents" ('Roy. Soc. Proc.,' vol. 45,

p. 270), I have shown that if Maxwell's theory that electricity moves like a perfectly incompressible fluid is not true, the rate of propagation of very rapidly alternating currents along a wire placed at an infinite distance from other conductors cannot be the same as the rate of propagation of the electrodynamic action through the surrounding dielectric. As Hertz, in his experiments on the rate of propagation of electrical waves along a metal wire, found that these rates were not the same, it might appear that this proved unmistakably that Maxwell's theory is untenable. I wish in this note to show that, assuming Maxwell's theory, we can explain the smaller velocity of propagation along wires found by Hertz, by taking into account the capacity of the wire, if the wire is not at a very great distance from other conductors; in fact that the capacity of the wire produces much the same effect as the "compressibility" of the electricity which is supposed to exist in all theories other than Maxwell's. In the case of a "free" wire in the laboratory, the electrical effects produced by the walls and floors are indefinite. I shall, therefore, consider on the most general theory the case of a wire surrounded by a coaxial metal cylinder, a case where the electrical conditions are perfectly definite if the electrical oscillations are very rapid.

I shall consider the case of a cylindrical wire surrounded by a cylindrical sheath of dielectric, which in its turn is surrounded by a third substance, either a conductor or another dielectric. The axis of the wire is taken as the axis of z, and all the variable quantities are supposed to vary as $e^{\iota(mz+pt)}$: the notation is as nearly as possible the same as the former paper. Let a be the radius of the wire, σ its specific resistance, b the outer radius of the sheath of dielectric, and K its specific inductive capacity; let us first suppose that this is surrounded by a substance whose specific resistance is σ'.

Let ϕ denote the electrostatic potential, and let

$$\phi = e^{\iota(mz+pt)}\mathrm{A}J_0(\iota qr), \text{ in the wire,}$$

$$= e^{\iota(mz+pt)}(\mathrm{B}J_0(\iota q'r)+\mathrm{C}I_0(\iota q'r)), \text{ in the dielectric,}$$

$$= e^{\iota(mz+pt)}\mathrm{D}I_0(\iota q''r), \text{ in the outer conductor;}$$

where $q^2 = m^2-\frac{p^2}{\omega^2}$, $\quad q'^2 = m^2-\frac{p^2}{\omega'^2}$, $\quad q''^2 = m^2-\frac{p^2}{\omega''^2}$,

where ω, ω', ω'' are the velocities of propagation of the electrostatic potential in the wire, sheath, and outer conductor respectively; in Maxwell's theory these are all infinite, and $q = q' = q'' = m$.

If F, G, H are the components of the vector potential, we may put

$$H = e^{\iota(mz+pt)}EJ_0(\iota nr)+\frac{\nu}{\iota p}\frac{d\phi}{dz}, \text{ in the wire,}$$

$$= e^{\iota(mz+pt)}(FJ_0(\iota\kappa r)+GI_0(\iota\kappa r))+\frac{\nu'}{\iota p}\frac{d\phi}{dz}, \text{ in the dielectric sheath,}$$

$$= e^{\iota(mz+pt)}LI_0(\iota n'r)+\frac{\nu''}{\iota p}\frac{d\phi}{dz}, \text{ in the outer conductor;}$$

where

$$n^2 = m^2+\frac{4\pi\iota p\mu}{\sigma}, \qquad \kappa^2 = m^2-\frac{p^2}{v^2}, \qquad n'^2 = m^2+\frac{4\pi\iota p\mu'}{\sigma'},$$

v being the velocity of propagation of electrodynamic action through the sheath, μ and μ' the magnetic permeabilities of the wire and outer conductor respectively, and ν and ν' constants.

$$\left.\begin{aligned} F &= \frac{d\chi}{dx}+\frac{\nu}{\iota p}\frac{d\phi}{dx} \\ G &= \frac{d\chi}{dy}+\frac{\nu}{\iota p}\frac{d\phi}{dy} \end{aligned}\right\} \text{ in the wire,}$$

where
$$\chi = \frac{\iota m}{n^2}e^{\iota(mz+pt)}EJ_0(\iota nr);$$

$$\left.\begin{aligned} F &= \frac{d\chi'}{dx}+\frac{\nu'}{\iota p}\frac{d\phi}{dx} \\ G &= \frac{d\chi'}{dy}+\frac{\nu'}{\iota p}\frac{d\phi}{dy} \end{aligned}\right\} \text{ in the dielectric,}$$

where
$$\chi' = -\frac{\iota m}{\kappa^2}e^{\iota(mz+pt)}(FJ_0(\iota\kappa r)+GI_0(\iota\kappa r));$$

and
$$\left.\begin{aligned} F &= \frac{d\chi''}{dx}+\frac{\nu''}{\iota p}\frac{d\phi}{dx} \\ G &= \frac{d\chi''}{dx}+\frac{\nu''}{\iota p}\frac{d\phi}{dy} \end{aligned}\right\} \text{ in the outer conductor,}$$

where
$$\chi'' = \frac{\iota m}{n'^2}LI_0(\iota n'r).$$

From the continuity of ϕ at the surfaces $r = a$, $r = b$, we have

$$\left.\begin{aligned} AJ_0(\iota qa) &= BJ_0(\iota q'a)+CI_0(\iota q'a) \\ DI_0(\iota q''b) &= BJ_0(\iota q'b)+CI_0(\iota q'b) \end{aligned}\right\} \quad \text{..........} \quad (1).$$

From the continuity of H we get

$$\left.\begin{aligned} EJ_0(\iota na) &= FJ_0(\iota\kappa a)+GI_0(\iota\kappa a)+\frac{\nu'-\nu}{\iota p}\iota mAJ_0(\iota qa) \\ LI_0(\iota n'b) &= FJ_0(\iota\kappa b)+GI_0(\iota\kappa b)+\frac{\nu'-\nu''}{\iota p}\iota mDI_0(\iota q''b) \end{aligned}\right\} \ldots (2).$$

From the continuity of F and G we get

$$\left.\begin{aligned} \frac{m}{n}EJ_0'(\iota na) &= \frac{m}{\kappa}(FJ_0'(\iota\kappa a)+GI_0'(\iota\kappa a))+\frac{\nu'\iota q'}{\iota p}(BJ_0'(\iota q'a) \\ &\qquad +CJ_0'(\iota q'a))-\frac{\nu\iota q}{\iota p}AJ_0'(\iota qa), \\ \frac{m}{n'}LI_0'(\iota n'b) &= \frac{m}{\kappa}(FJ_0'(\iota\kappa b)+GI_0'(\iota\kappa b))+\frac{\nu'\iota q'}{\iota p}(BJ_0'(\iota q'b) \\ &\qquad +CJ_0'(\iota q'b))-\frac{\nu''\iota q''}{\iota p}DJ_0'(\iota q''b). \end{aligned}\right\} \ldots\ldots (3).$$

The magnetic force parallel to the surface of the wire is

$$\frac{1}{\mu}\left(\frac{d}{dz}\frac{d\chi}{dr}-\frac{dH}{dr}\right),$$

and, since this is continuous, we have

$$\left.\begin{aligned} \frac{1}{\mu}\frac{m^2-n^2}{n}EJ_0'(\iota na) &= \frac{m^2-\kappa^2}{\kappa}(FJ_0'(\iota\kappa a)+GI_0'(\iota\kappa a)), \\ \frac{1}{\mu'}\frac{m^2-n'^2}{n'}LI_0'(\iota n'b) &= \frac{m^2-\kappa^2}{\kappa}(FJ_0'(\iota\kappa b)+GI_0'(\iota\kappa b)). \end{aligned}\right\} \ldots (4).$$

Eliminating E and L from equations (2) and (4), we get

$$\begin{aligned} &F\left(\frac{m^2-n^2}{\mu n}J_0(\iota\kappa a)J_0'(\iota na)-\frac{m^2-\kappa^2}{\kappa}J_0'(\iota\kappa a)J_0(\iota na)\right) \\ &+G\left(\frac{m^2-n^2}{\mu n}J_0'(\iota na)I_0(\iota\kappa a)-\frac{m^2-\kappa^2}{\kappa}J_0(\iota na)I_0'(\iota\kappa a)\right) \\ &=\frac{\nu-\nu'}{\iota p}\iota m\frac{m^2-n^2}{\mu n}AJ_0(\iota pa)J_0'(\iota na) \quad \ldots\ldots\ldots\ldots\ldots\ldots (5), \end{aligned}$$

$$\begin{aligned} &F\left(\frac{m^2-n^2}{\mu'n'}J_0(\iota\kappa b)I_0'(n\ b)-\frac{m^2-\kappa^2}{\kappa}J_0'(\iota\kappa b)I_0(\iota nb)\right) \\ &+G\left(\frac{m^2-n'^2}{\mu'n'}I_0(\iota\kappa b)I_0'(\iota n'b)-\frac{m^2-\kappa^2}{\kappa}I_0'(\iota\kappa b)I_0(\iota nb)\right) \\ &=\frac{\nu''-\nu'}{\iota p}\iota m\frac{(m^2-n'^2)}{\mu'n'}DI_0(\iota pb)I_0'(\iota n'b) \quad \ldots\ldots\ldots\ldots\ldots\ldots (6). \end{aligned}$$

Eliminating E and F from equations (3) and (4), we get

$$\{FJ_0'(\iota\kappa a)+GI_0'(\iota\kappa a)\}\frac{1}{\kappa}\left\{1-\frac{m^2-\kappa^2}{m^2-n^2}\mu\right\}$$

$$=\frac{\nu q}{pm}AJ_0'(\iota qa)-\frac{\nu' q'}{pm}(BJ_0'(\iota q'a)+CI_0'(\iota q'a)) \quad \ldots\ldots (7),$$

$$\{FJ_0'(\iota\kappa b)+GI_0'(\iota\kappa b)\}\frac{1}{\kappa}\left\{1-\frac{m^2-\kappa^2}{m^2-n^2}\mu'\right\}$$

$$=\frac{\nu'' q''}{pm}DI_0'(\iota q''b)-\frac{\nu' q'}{pm}(BJ_0'(\iota q'b)+CI_0'(\iota q'b)) \quad \ldots\ldots (8).$$

We can substitute for B and C from equations (1) and get four equations from which we can eliminate F, G, A, and D; as, however, the result is lengthy, we shall only solve it for the particular case with which we are concerned, when qa, $q'a$, $q'b$, and $q''b$, κa and κb, are all small. We get, substituting in the terms multiplied by $\nu'-\nu$, the approximate values of the Bessel's functions for small quantities of the variable

$$\left\{\frac{(m^2-n^2)}{\mu n}J_0(\iota\kappa a)J_0'(\iota na)-\frac{(m^2-\kappa^2)}{\kappa}J_0'(\iota\kappa a)J_0(\iota na)\right\}$$

$$\left\{\frac{(m^2-n'^2)}{\mu' n'}I_0(\iota\kappa b)I_0'(\iota n'b)-\frac{(m^2-\kappa^2)}{\kappa}I_0'(\iota\kappa a)I_0(\iota nb)\right\}$$

$$=\left\{\frac{m^2-n^2}{\mu n}I_0(\iota\kappa a)J_0'(\iota na)-I_0'(\iota\kappa a)\left[\frac{m^2-\kappa^2}{\kappa}J_0(\iota na)\right.\right.$$

$$\left.\left.-\iota\frac{(\nu'-\nu)}{\nu'}\frac{m^2}{\kappa}\frac{m^2-n^2}{\mu n}aJ_0'(\iota na)\log\frac{b}{a}\right]\right\}$$

$$\times\left\{\frac{m^2-n'^2}{\mu n'}J_0(\iota\kappa b)I_0'(\iota n'b)-\frac{m^2-\kappa^2}{\kappa}J_0'(\iota\kappa b)I_0(\iota nb)\right\}.$$

In this equation the approximate values of the Bessel's functions have been used only in the term multiplied by $\nu'-\nu$.

This equation simplifies very much, since κa and κb are very small, and therefore approximately

$$J_0(\iota ka)=1, \qquad J_0'(\iota ka)=-\tfrac{1}{2}\iota ka, \qquad I_0(\iota ka)=\log\gamma i ka,$$
$$\text{and } I_0'(\iota ka)=1/\iota ka.$$

Making these substitutions, the above equation reduces to

$$\kappa^2=\iota\frac{p^2}{v^2}\left\{\frac{\mu n}{m^2-n^2}\frac{1}{a}\frac{J_0(\iota na)}{J_0'(\iota na)}-\frac{\mu' n'}{m^2-n'^2}\frac{1}{b}\frac{I_0(\iota nb)}{I_0'(\iota n'b)}\right\}\frac{1}{\log(b/a)}+\frac{\nu'-\nu}{\nu'}m^2,$$

(see also "Electrical Oscillations on Cylindrical Conductors," 'London Math. Soc. Proc.,' vol. 17, p. 320),

or,

$$m^2\frac{\nu}{\nu'} = \frac{p^2}{v^2}\left\{1+\iota\left(\frac{\mu n}{m^2-n^2}\frac{1}{a}\frac{J_0(\iota na)}{J_0'(\iota na)} - \frac{\mu' n'}{m^2-n'^2}\frac{1}{b}\frac{I_0(\iota n'b)}{I_0'(\iota n'b)}\right)\frac{1}{\log(b/a)}\right\} \text{ (9).}$$

The nature of the solution of this equation will depend upon the magnitudes of na and $n'b$.

Case I. na and $n'b$ both small. In this case

$$J_0(\iota na)/J_0'(\iota na) = -2/\iota na \quad \text{and} \quad I_0(\iota n'b)/I_0'(\iota n'b) = \iota n'b \log \gamma\iota n'b.$$

Making these substitutions, equation (9) becomes

$$m^2\frac{\nu}{\nu'} = \frac{p^2}{v^2}\left\{1-\frac{\log\gamma\,\iota n'b}{\log(b/a)}+\iota\frac{\sigma}{2\pi a^2 p}\frac{1}{\log(b/a)}\right\} \text{ ......(10).}$$

Since na is by hypothesis small, $\sigma/2\pi a^2 p$ is large compared with unity, and unless $\log(b/a)$ is very great, it will be much the largest term inside the bracket, so that (10) may be written

$$m^2\frac{\nu}{\nu'} = \frac{p^2}{v^2}\frac{\iota\sigma}{2\pi a^2 p}\frac{1}{\log(b/a)},$$

$$m = \frac{p}{v}\sqrt{\left\{\frac{\nu'\sigma}{\nu 4\pi a^2 p\log(b/a)}\right\}}\cdot(1+\iota).$$

This represents a disturbance propagated with the velocity

$$v\left\{\left(\frac{4\ \nu a^2 p}{\sigma\nu'}\right)\log(b/a)\right\}^{\frac{1}{2}},$$

and dying away to $1/e$ of its original value after traversing a distance

$$\left\{\left(\frac{4\pi\nu a^2}{\nu'\sigma p}\right)\log(b/a)\right\}^{\frac{1}{2}}.$$

This case, which is that of slowly alternating currents, was solved many years ago by Sir William Thomson.

Case II. na large, $n'b$ small. This is the case of rapidly alternating currents travelling along a wire which is surrounded by a substance whose conductivity is so small that $4\pi\mu' pb^2/\sigma'$ is a small quantity.

In this case, since $J_0'(\iota na) = \iota J_0(\iota na)$, equation (9) reduces to

$$m^2\frac{\nu}{\nu'} = \frac{p^2}{v^2}\left\{1-\frac{\mu}{na}\frac{1}{\log(b/a)}-\frac{\log\gamma\,\iota n'b}{\log(b/a)}\right\} \text{ ......(11).}$$

Since na is large, the second term in the bracket will be small for wires made of non-magnetic metals; so that for this case (11) reduces to

$$m^2\frac{\nu}{\nu'}=\frac{p^2}{v^2}\left\{1+\frac{\log \gamma\iota n'b}{\log (b/a)}\right\},$$

or, substituting for n' the approximate value $\sqrt{\left(\frac{4\pi\mu'\iota p}{\sigma'}\right)}$,

$$m^2\frac{\nu}{\nu'}=\frac{p^2}{v^2}\left\{1+\tfrac{1}{2}\frac{\log (\sigma_1/4\pi\mu' pb^2)}{\log (b/a)}+\frac{\iota(\pi/4)}{\log (b/a)}\right\}\text{ approximately,}$$

or

$$m=\frac{p}{v}\left\{\frac{\nu'}{\nu}\right\}^{\frac{1}{2}}\left\{\frac{\log (\sigma'/4\pi\mu' pa^2)}{\log (b^2/a^2)}\right\}^{\frac{1}{2}}\left\{1+\iota\frac{\pi}{4}\frac{1}{\log (\sigma'/4\pi\mu' pa^2)}\right\}.$$

This represents a disturbance propagated with the velocity

$$v\sqrt{\frac{\nu}{\nu'}}\cdot\frac{1}{\sqrt{\left(1+\frac{\log (\sigma'/4\pi\mu' pb^2)}{\log (b^2/a^2)}\right)}},$$

and fading away to $1/e$ of its original value, after traversing a distance

$$\frac{4}{\pi}\frac{v}{p}\sqrt{\frac{\nu}{\nu'}}\sqrt{\left(\log\frac{b^2}{a^2}\log\frac{\sigma'}{4\pi\mu' pa^2}\right)},$$

or if λ is the wave-length of the electrical vibration, the distance a disturbance travels before falling to $1/e$ of its original value is

$$\frac{2\lambda}{\pi^2}\log\frac{\sigma'}{4\pi\mu' pa^2}.$$

Thus in this case, even if ν' equals ν, that is, if Maxwell's theory is correct, the rate of propagation of the disturbance along the wire will not be the same as that of electrodynamic action through air; and yet the conditions may be such as to allow a disturbance to pass over several wave-lengths before falling to $1/e$ of its original value. It will be noticed that the velocity of propagation does not depend on the specific resistance of the wire, and that it increases with the rapidity of the reversal, and that the rate at which the vibrations die away is independent of the resistance of the wire, and only varies slowly with the resistance of the outer conductor, since σ' only enters in the form $\log \sigma'$.

We can see the reason of this if we consider the amount of heat produced in the outer conductor.

If i is the current parallel to the axis of z passing through a section of the wire, then, assuming in the investigation that $\nu'=\nu$,

$$i = \frac{\iota p}{\sigma} \mathrm{E} e^{\iota(mz+pt)} \int_0^a 2\pi r \mathrm{J}_0(\iota nr) dr,$$

$$= \frac{\iota p}{\sigma} e^{\iota(mz+pt)} \mathrm{E} \frac{2\pi}{n^2} \iota na \mathrm{J}_0'(\iota na).$$

The rate of production of heat in the wire is

$$\frac{p^2}{\sigma} \mathrm{E}^2 e^{2\iota(mz+pt)} \int_0^a 2\pi r (\mathrm{J}_0(\iota nr))^2 dr$$

$$= \frac{p^2}{\sigma} \mathrm{E}^2 \pi a^2 \{\mathrm{J}_0'^2(\iota na) + \mathrm{J}_0^2(\iota na)\} e^{2\iota(mz+pt)}.$$

Hence the ratio of the heat generated in the wire to $\sigma i^2/\pi a^2$, the heat which would be generated if the current were uniformly distributed,

$$= \tfrac{1}{4} n^2 a^2 \frac{\{\mathrm{J}_0'^2(\iota na) + \mathrm{J}_0^2(\iota na)\}}{\mathrm{J}_0'^2(\iota na)}.$$

Since when na is large $\mathrm{J}_0(\iota na) = (e^{na}/\sqrt{2\pi na})\left(1 + \frac{1}{8na}\right)$, this ratio

$$= \tfrac{1}{4} na.$$

The rate at which heat is generated in the outer conductor is

$$\frac{p^2}{\sigma'} e^{2\iota(mz+pt)} \mathrm{L}^2 \int_b^\infty 2\pi r \mathrm{I}_0^2(\iota n' r) dr,$$

$$\frac{p^2}{\sigma'} \mathrm{L}^2 \pi b^2 \{\mathrm{I}_0'^2(\iota n' b) + \mathrm{I}_0^2(\iota n' b)\} e^{2\iota(mz+pt)}.$$

By equations (7) and (8) we have

$$\frac{\mathrm{L}\mathrm{I}_0(\iota n' b)}{\mathrm{E}\mathrm{J}_0(\iota na)} = \frac{a}{b} \frac{m^2 - n^2}{\mu n} \frac{\mu' n'}{m^2 - n'^2} \frac{\mathrm{J}_0'(\iota na)}{\mathrm{J}_0(\iota na)} \frac{\mathrm{I}_0(\iota n' b)}{\mathrm{I}_0'(\iota n' b)},$$

so that $$\pi \mathrm{L} b \mathrm{I}_0'(\iota n' b) e^{\iota(mz+pt)} = \tfrac{1}{2} \frac{m^2 - n^2}{\mu} \frac{\mu' n'}{m^2 - n'^2} \cdot \frac{\sigma i}{p}$$

$$= \frac{2\pi\mu'}{n'} \cdot i, \text{ approximately.}$$

Thus the heat generated in the outer conductor

$$= 4 \frac{\pi p^2}{\sigma'} i^2 \cdot \frac{\mu'^2}{n_1^2} \left\{1 + \frac{\mathrm{I}_0^2(\iota n' b)}{\mathrm{I}_0'^2(\iota n' b)}\right\},$$

since $n'b$ is small, $\mathrm{I}_0'(\iota n' b)$ is large compared with $\mathrm{I}_0(\iota n' b)$, and n'^2 is approximately

$$4\pi\mu' \iota p/\sigma'.$$

Thus the rate at which heat is generated in the outer conductor is approximately

$$\mu' p i^2,$$

and is therefore approximately independent of the resistances of the wire and of the outer conductor, and large compared with the heat developed in the wire.

The case of an iron wire would differ from that investigated in the case when though na is large, μ/na is also large; in this case equation (9) becomes approximately

$$m^2 \frac{\nu}{\nu'} = \frac{p^2}{v^2}\left\{1 - \frac{\mu}{na}\right\},$$

which represents a vibration travelling with a smaller velocity than that of the electrodynamic action through the dielectric, and dying away to $1/e$ of its original value after traversing a space comparable with a wave-length. When the rate of alternation of the currents gets sufficiently rapid, $n'b$ gets large, and μ/na small, and we get

Case III. na and $n'b$ both large.

In this case, since $J_0'(\iota na) = \iota J_0(\iota na)$ and $I_0'(\iota n'b) = -\iota I_0(\iota n'b)$ and equation (9) reduces to

$$m^2 \frac{\nu}{\nu'} = \frac{p^2}{v^2}\left\{1 - \left(\frac{\mu}{na} + \frac{\mu'}{n'b}\right)\frac{1}{\log(b/a)}\right\}$$

$$= \frac{p^2}{v^2}\left\{1 + \frac{\iota}{\sqrt{(8\pi p)}}\left\{\sqrt{\left(\frac{\sigma\mu}{\mu a^2}\right)} + \sqrt{\left(\frac{\sigma'\mu'}{b^2}\right)}\right\}\frac{1}{\log(b/a)}\right\}.$$

$$m = \frac{p}{v}\sqrt{\frac{\nu}{\nu'}}\left\{1 + \frac{\iota}{2\sqrt{(8\pi p)}}\left\{\sqrt{\left(\frac{\sigma\mu}{a^2}\right)} + \sqrt{\left(\frac{\sigma'\mu'}{b^2}\right)}\right\}\frac{1}{\log(b/a)}\right\}.$$

This represents a vibration travelling with the velocity $v\sqrt{(\nu/\nu')}$, and dying away to $1/e$ of its original value after traversing a distance

$$4v\sqrt{\frac{\nu}{\nu'}}\sqrt{\frac{2\pi}{p}}\left\{\sqrt{\frac{\sigma\mu}{a^2}} + \sqrt{\frac{\sigma'\mu'}{b^2}}\right\}^{-1}\log(b/a).$$

From this equation we see that if σ/a^2 is very much greater than σ'/b^2, the decay of the disturbance will be due chiefly to the resistance of the wire, but if, on the other hand, σ'/b^2 is very much greater than σ/a^2, the decay will be due chiefly to the resistance of the outer conductor. This case includes that of a wire surrounded by a metal tube, the space between the tube and the wire being occupied by any dielectric, in this case the electrical conditions are perfectly definite, and we see that the velocity of propagation along the wire will be $v\sqrt{(\nu/\nu')}$, where v is the velocity of propagation of the electrodynamic action through the dielectric. Thus if $\nu' = \nu$, as in Maxwell's theory, the velocity along the wire will be the same as that through the

dielectric, but it will not be so unless this condition is fulfilled. Thus this case would afford a definite means of testing whether or not Maxwell's theory is true. The thickness of the outer tube would be immaterial, as with these very rapid vibrations the currents are entirely confined to the inner skin of the tube.

By comparing the results for this case with those of Case II, we see that if the rate at which the electrical disturbances die away depends on the conductivity of the wire, the velocity of propagation through the wire must be the same as that through the dielectric if Maxwell's theory is true.

The preceding equations can be modified so as to include the case when the outer conductor is replaced by another dielectric; all that we have to do is in equation (9) to replace n' by κ', where

$$\kappa'^2 = m^2 - (p^2/v_1^2),$$

v_1 being the velocity of propagation through the outer dielectric.

In this case equation (9) becomes

$$\kappa^2 = \frac{\iota p^2}{v^2}\left\{\frac{\mu n}{m^2-n^2}\frac{J_0(\iota na)}{J_0'(\iota na)} - \frac{\kappa'}{m^2-\kappa_1^2}\frac{I_0(\iota\kappa' b)}{I_0'(\iota\kappa' b)}\right\}\frac{1}{\log(b/a)} + \frac{\nu'-\nu}{\nu'}m^2.$$

If both $\kappa' b$ and na are large, the velocity is the same as before, viz., $v\sqrt{(\nu/\nu')}$. If na is large and $\kappa' b$ small, the equation becomes approximately

$$\frac{\nu}{\nu'}m^2 - \frac{p^2}{v^2} = \kappa_1^2\frac{I_0(\iota\kappa' b)}{\log(b/a)}\frac{v_1^2}{v^2},$$

or substituting for $I_0(\iota\kappa' b)$ we get

$$m^2\left\{\frac{\nu}{\nu'} + \frac{\log(1/\gamma\kappa' b)}{\log(b/a)}\frac{v_1^2}{v^2}\right\} = \frac{p^2}{v^2}\left\{1 + \frac{\log(1/\gamma\kappa' b)}{\log(b/a)}\right\}.$$

Thus the velocity of propagation is

$$\frac{\left\{v^2\frac{\nu}{\nu'} + \frac{\log(1/\gamma\kappa' b)}{\log(b/a)}v_1^2\right\}^{\frac{1}{2}}}{\left\{1 + \frac{\log(1/\gamma\kappa' b)}{\log(b/a)}\right\}^{\frac{1}{2}}}.$$

As it has been shown above that if the rate at which the vibrations decay depends upon the nature of the wire, the rate of propagation of the disturbance along the wire will be $v\sqrt{(\nu/\nu')}$, I thought it would be of interest to determine the rate of propagation in this case, in order to see whether the velocity would still differ as much as in Hertz's experiments from that of the propagation of the electrodynamic action through air.

The method used is shown in fig. 1. AB, CD is the action of a vibrator (shown in elevation in fig. 2) of the same shape and size as the one used by Hertz in the experiments described in Wiedemann's 'Annalen,' vol. 34, p. 553, AB, CD being squares of tin-plate, 40 cm. square. BE, EF wires, each 30 cm. long, terminating in the brightly polished balls E and F, these balls being separated by an air space of 3 or 4 mm. The terminals of the induction coil are fastened to BE, CF respectively. L, N are pieces of tin-plate placed in front of AB and CD, having insulated wires about 25 metres long fastened to them, the ends M and O being covered with sealing-wax.

The resonator (fig. 3) is, as in Hertz's experiments, a ring of wire about 70 cm. in diameter, terminating in two balls, the distance between which can be accurately adjusted by means of a screw. The way in which the resonator was used was different from Hertz's method. Two wires of equal length covered with gutta-percha and

surrounded by tin-foil, connected at both ends with the earth, were fastened close to the balls of the resonator; the other extremities of these wires could move along the wires LM, NO respectively. When the coil was working, sparks passed between the balls of the resonator, and it was found that the intensity of these sparks depended on the position of the points P and Q, to which the extremities of the wires of the resonator were attached. The experiments made to determine the velocity through the wire were as follows: the end Q of one of the wires of the resonator was placed at O, the end of the wire NO and the extremity P of the other wire moved along LM until the sparks in the resonator were as faint as possible; the distance P_1M, when this was the case, was about 5 metres. We may conclude that in this position the points P_1 and O are nearly at the same potential. The end of the other wire was then moved along NO until the sparks were again as faint as possible; the position Q_1, when this was the case, was such that Q_1O was between 10 and 10·25 metres. Since the sparks are again a minimum, we may conclude that P_1 and Q_1 are again at nearly the same potential, hence the potentials at Q_1 and O must be very nearly equal, but when this is the case, Q_1O must be very nearly a wave-length; the wave-length in the wire LM was found in a similar way to be also about 10 metres. Hence the wave-length of the electrical vibration in the wire must in this case be about 10 metres, but Hertz has shown by the interference of the direct electrical waves, and those reflected from a large metal reflector, that the wave-length of the action propagated through the air from this vibrator is also about 10 metres, and the length of the wave must be approximately the same in our experiments as the resonator which responded to the vibrations was of the same dimensions. So that in this case the velocity of propagation through the wire is the same as that through the air.

Since the sparks between the balls of the resonator never actually vanish, the determination of the places where they are as faint as possible is a matter of judgment, and thus the method is not capable of any very great accuracy. I found, however, on comparing my results with those of another observer, Mr. E. Everett, that the two sets agreed within about 2 feet in 10 metres.

The rate at which the disturbances die away was determined by a preliminary experiment. In this only one wire was used, and this was carried over the laboratory until when the resonator was used in the way described by Hertz no sparks passed between the balls; the length of wire necessary for this was more than twice as great for copper as for German silver wire of the same diameter. Thus the rate of decay depends on the material of which the wire is made, and, therefore, by the above investigation the velocity through the wire is $v\sqrt{(\nu/\nu')}$.

[On repeating the experiments after the Easter Vacation, an effect was observed which may explain the difference between the value of the wave-length along the wire found in the above experiments and those of Hertz. It happened that the plates after the vacation were placed further from the wall than they had been before, and it was found that the wave-length was much less, being now between seven and eight metres; on moving the plates nearer the wall the wave-length increased, the increase being evidently due to the increase in the capacity of the plate produced by the proximity of the wall. Thus if the distance of the plates from the walls was different in the determination of the wave-length along the wire from what it was in the determination of the wave-length through air, the wave-lengths would not be equal even if the velocity of propagation were the same. I endeavoured to determine the wave-length in air by measuring the distance between the nodes after reflection from a large metal screen, but could not succeed in fixing the position of the nodes with sufficient definiteness to determine the wave-length with any accuracy. The fact, however, that I got a wave-length in the wire the same as that obtained by Hertz through air, is sufficient to show that it is not necessary to suppose that the velocities through the wire and air are different, but that the difference in Hertz's results may have been due to a change in the position of the vibrator relatively to the walls of the room.—May 15.]

II. "Researches in the Chemistry of Selenic Acid and other Selenium Compounds" By Sir CHARLES A. CAMERON, M.D., F.R.C.S.I., V.P.I.C., Professor of Chemistry and Hygiene, R.C.S.I., and JOHN MACALLAN, F.I.C., Demonstrator of Chemistry, R.C.S.I. Communicated by Sir HENRY ROSCOE, F.R.S. Received April 6, 1889.

Although selenic acid was prepared by Mitscherlich so far back as the year 1827, few chemists appear to have studied its properties. This want of interest in selenic acid is rather surprising, seeing that it possesses so close a relationship to sulphuric acid, which is so important a compound. Finding the chemistry of selenic acid so meagre, we resolved to make an investigation of this body, with the view of bringing, so far as we could, its chemistry abreast with that of sulphuric acid, and also in the hope that its study would yield results which might throw additional light on the relations of the latter acid. The following pages contain the results at which we have arrived.

Preparation of Anhydrous Selenic Acid, H_2SeO_4.

Selenic acid has hitherto been known only in a dilute form. When heated to about 260° C. it commences to decompose into selenium dioxide, oxygen, and water, which prevents any further concentration. Berzelius describes it as containing, when of greatest strength, 4 per cent. of water; but since his time it has been obtained in a more concentrated condition by Fabian, who, by evaporating the acid to a temperature of 265°, found it to have a strength of 94·9 per cent., and by placing this acid, while still hot, under the receiver of an air-pump, increased its strength to 97·4 per cent. Sulphuric acid, as is well known, has not been obtained in a perfectly anhydrous state by ebullition—although in its case decomposition does not occur; for when it reaches a strength of about 98·66 per cent. it boils without further change. When, however, an acid of this strength is surrounded with a freezing mixture, the anhydrous acid, H_2SO_4, crystallises out. An attempt was first made to obtain anhydrous selenic acid by similar means. Great care was taken to obtain an acid of pure quality. An examination was specially made for nitric, sulphuric, hydrochloric, and hydrobromic acids. Selenious acid, when present, was removed by diluting with about 30 parts of water, saturating with hydrogen sulphide in the cold, filtering and concentrating on the water-bath. The acid thus treated was then examined for sulphuric acid and found to contain none. A portion of the acid, when ignited, left a residue equivalent to 0·07 per cent. of the anhydrous acid present in the specimen used in the experiments; this was ascertained to consist of neutral sodium selenate, and was, of course, derived from an acid salt, of which it was necessary to take account in the succeeding experiments.

Some of this acid was gradually heated until the temperature rose to 250°. It was next poured into an open dish, and allowed to cool slowly over sulphuric acid, under an exhausted receiver. The strength was then taken with seminormal soda solution, which was specially prepared for those experiments, by making a solution in water of pure caustic hydrate prepared from sodium, and bringing it carefully to the required strength: 11·81 cubic centimetres of soda solution were required for neutralisation by 0·4371 gram of acid, equivalent to 97·75 per cent. of selenic acid, being thus a little stronger than that obtained by Fabian in a similar way.

The acid thus concentrated was then poured into a stout wide glass tube, having one end closed, and the open end fitted with an india-rubber cork, through which passes a glass rod for the purpose of keeping the viscous liquid stirred, and a capillary tube for admission of air, in order to expose the acid to the full atmospheric pressure. The necessity for those precautions in the case of sulphuric acid has

already been fully pointed out by Marignac ('Annales de Chimie,' vol. 39, 1853, p. 184), the discordant results arrived at by various observers being probably due to the different conditions under which they worked. The latter precaution is particularly necessary, as we have observed that sulphuric acid occupies less volume in the solid than in the liquid state.

The temperature of the acid was then gradually lowered. Its viscosity increased as it became colder, until at a temperature of —51·5° C. it was as thick as soft pitch—the tube might be inverted without the acid flowing, and a glass rod could be moved in it only with great difficulty. Still it did not freeze, until after vigorous stirring maintained for a couple of minutes, a crystal appeared, and then the entire mass almost instantaneously crystallised, the temperature rapidly rising. When removed to a warm room, the crystals were rather permanent, and when nearly melted, recrystallisation could easily be induced by exposure to moderate cold so long as any crystal remained, showing that previous to freezing the acid had been in a more or less superfused condition. It was found to be impossible, however, to separate the crystals by draining the liquid portion, as the latter was so viscous that it carried the crystals with it. Under the microscope they were seen to be long prisms.

An attempt was next made to examine the conditions under which selenic acid becomes strengthened in a vacuum, with the object of obtaining, if possible, a more concentrated acid; and the following arrangement was made for the purpose:—The glass receiver of an air-pump was fitted tightly at its neck with an india-rubber cork, through which passed a bent tube, connected horizontally with another and wider tube containing solid potash. This was connected with a wide U-tube, filled with pieces of potash about half an inch long, and kept cool by immersion in a beaker of water. Connexion was then made with a small stout flask containing the selenic acid, and varying in size from 100 to 250 cubic centimetres as required. The potash-tube next the receiver was intended for the purpose of preventing acid fumes from injuring the air-pump, and for the same reason a vessel of potash was placed within the receiver. The air-pump employed gave a very good vacuum; when all the connexions were made, and the pump exhausted, there was often scarcely any appreciable difference in the levels of the columns of mercury in the gauge. For temperatures up to 100° the flask containing the acid was heated in a beaker of water; for higher temperatures oil was used. Soon after commencing the experiments it was found necessary to make an arrangement for the purpose of stirring up the viscous acid and exposing fresh surfaces to the vacuum, and the following plan was devised:—A test-tube, which fitted easily the neck of the flask, was shortened by removing evenly a portion of the open end. It was

then inverted and pushed down into the flask, so that when the open end was below the surface of the acid, the closed end extended sufficiently far into the neck of the flask to prevent the tube from being thrown down by the ebullition of the liquid. The vapour given off from the acid within the tube became gradually expanded as the temperature rose, and passed in a stream of bubbles through the acid, keeping it well agitated. The tube also served the purpose of preventing splashing from the boiling acid up into the neck of the small flask.

Some selenic acid, which had been previously partially concentrated, was kept in an open dish on the water-bath for four hours and its strength then determined: 0·6364 gram required for neutralisation 14·72 c.c. of seminormal soda solution, equivalent to 83·68 per cent. of selenic acid—a strength intermediate between a monohydrate and a dihydrate. The acid thus obtained at 100°, under the ordinary pressure of the atmosphere, was stronger than that obtained by Graham from dilute sulphuric acid by heating it to the same temperature in a vacuum until it ceased to lose weight—the dihydrate, $H_2SO_4,2H_2O$, remaining showing the greater affinity for water possessed by the latter acid.

Selenic acid, concentrated as above described, was poured into the flask previously referred to, and gradually heated. Weak selenic acid commenced to pass over at 56°, evidenced by the potash liquefying and effervescing, owing to the presence of potassium carbonate which it contained, being the ordinary commercial potash. It was then heated slowly up to 100°, kept at that temperature so long as any acid distilled over, and the strength of the residue taken: 0·9432 gram required for neutralisation 24·10 c.c. of seminormal soda solution, equivalent to 92·44 per cent. of selenic acid. Subsequent to this experiment, acids heated on various occasions to 100° were found to have the following percentages of anhydrous selenic acid:—92·03, 92·08, 93·28, and 93·70; the different results being found due to the varying conditions of the experiments, such as the length of time of heating, the quantity of potash, and its proximity to the acid, the amount of the latter, and the size of the flask containing it.

The acid which had been heated to 100°, was next heated to 150°. At the latter temperature the more or less dilute acid distilling over appeared in the form of dense white fumes resembling those of sulphuric acid. 0·6898 gram of the residue left neutralised 18·42 c.c. of seminormal soda solution, equivalent to 96·58 per cent. of selenic acid. It was again heated to 150°, and kept at that temperature so long as any acid distilled over: 0·417 gram of the residue left neutralised 11·21 c.c. of seminormal soda, equivalent to 97·25 per cent. of selenic acid.

A fresh portion of acid was heated to 155° and kept for some time at that temperature: 0·8045 gram of the residue neutralised 21·72 c.c.

of seminormal soda, showing 97·67 per cent. of selenic acid. It was next heated to 162°, and the strength of the residue taken: 0·7291 gram neutralised 19·72 c.c. of seminormal soda, equivalent to 97·85 per cent. of selenic acid.

The same acid was then heated to 216°, and the residue obtained was allowed to remain in the flask during the night. In the morning it was found to be frozen into a crystalline mass so hard that it was necessary to use a steel chisel in order to remove portions for examination. When dissolved in water and tested it was found to contain some selenium dioxide. Trials were then made to ascertain if a lower temperature would produce a similar result without decomposition of the acid; 180° was found to be sufficient for the purpose, and the following course was finally adopted:—The acid, which had been concentrated on the water-bath as far as possible, was heated gradually in the flask to 100°, and kept at that temperature so long as any acid distilled over, the greater part of the water being thus removed. The U-tube was then disconnected, emptied, and refilled with stick potash. The flask was next heated gradually to 180°, kept at that temperature until no more acid distilled over, and then immediately cooled. A still better arrangement was to use so little acid that it was unnecessary to change the potash. The acid was heated gradually and continuously up to 180°, allowing bubbles to pass slowly through it, as before described. When 180° was reached the potash was watched, and as soon as it ceased to be acted upon the flask was immediately cooled. An hour or less was generally found sufficient time for a small quantity of acid.

A specimen obtained in this way was found, when examined, to be very free from selenium dioxide, a little of it diluted with water and saturated with hydrogen sulphide, merely giving a faint yellow coloration without any precipitate. Another portion was acidified with hydrochloric acid and barium chloride added; on boiling the filtrate with stannous chloride it only became darkened in colour without any precipitation of selenium. 0·6725 gram was taken to estimate the strength: 18·54 c.c. of seminormal soda were required for neutralisation, equivalent to 99·73 per cent. of selenic acid. 0·724 gram of another acid, prepared in a similar way, but which contained rather more selenium dioxide than the last, required for neutralisation 19·94 c.c. of seminormal soda, equivalent to 99·64 per cent. of selenic acid. A portion of a third acid, weighing 0·329 gram, was dissolved in water, barium chloride added, and also hydrochloric acid in order to prevent any selenious acid from precipitating. The resulting barium selenate weighed 0·6337 gram, equivalent to 99·75 per cent. of selenic acid. As has been already mentioned, the acid employed contained an acid potassium selenate equivalent to 0·07 per cent. of neutral sodium selenate found. Taking this into account, and calcu-

lating on the acid with the sodium selenate deducted, the three results become respectively—

I.	II.	III.
99·80	99·71	99·77

The difference between the above results and 100 per cent. must be ascribed partly to the very hygroscopic character of the acid in the anhydrous condition, and consequent slight absorption of moisture during the process of weighing. It is necessary to observe, however, that in making the above calculations, and all through in this paper, 78·87 has been adopted as the atomic weight of selenium, the number given by Meyer and Seubert in their 'Recalculations of the Atomic Weights.' If 78·80, the number given in Clarke's 'Recalculations,' be taken, the three results become—

I.	II.	III.
99·75	99·66	99·72

Petersson and Ekman state that the results of a great many analyses show that the most probable atomic weight of selenium is 79·08 ('Berichte Deutsch. Chem. Gesell.,' vol. 9, p. 1210). If this number be taken, the above percentages become respectively—

I.	II.	III.
99·94	99·85	99·91

The results arrived at from the foregoing experiments lead to the conclusion that *at* 180° *in a vacuum, selenic acid parts with all combined water, and remains as the anhydrous acid,* H_2SeO_4.

It may be well here to summarise the precautions necessary to be taken in preparing the anhydrous acid, so far as we have ascertained them. They are briefly as follows:—To use for the purpose an acid as pure as possible, to have a thoroughly good vacuum, to avoid too high or prolonged heating, and to keep a sufficient quantity of solid potash in close proximity to the acid all through—which may be arranged by using a small flask, and having the tube leading from it short and wide.

Properties of Anhydrous Selenic Acid.

Anhydrous selenic acid is a white crystalline solid melting at 58° to a colourless oily-looking liquid. When thoroughly melted it remains in a superfused state, and usually requires to be cooled to about 5° with constant stirring before it again freezes. The temperature then rises rapidly to 58°, and remains stationary until complete solidification of the acid has taken place. When at rest it can be cooled lower than 5° without freezing, and it will remain in a stoppered bottle for months, and during frosty weather, in a liquid condition.

It instantly solidifies at any temperature below 58° if a crystal of the solid acid be dropped into it, and it freezes sometimes at ordinary temperatures when rubbed with a sharp piece of glass or with the point of a pipette. It thus exhibits the property of superfusion in a remarkable degree, and to a greater extent than anhydrous sulphuric acid, which, according to Marignac, possesses eminently the property of superfusion ('Annales de Chimie,' vol. 39, 1853, p. 184). Its melting point, 58°, is higher than that of anhydrous sulphuric acid, 10·5°, but lower than that of telluric acid, which may be heated nearly to redness without melting.

Anhydrous selenic acid crystallises in long interlacing hexagonal prisms. In an impure condition from the presence of selenium dioxide and other substances, its melting point is lowered, and under those circumstances it is sometimes deposited slowly and spontaneously in the form of double pyramids, many of them intersecting in pairs.

Although much has been written upon the freezing-point of sulphuric acid, but little information appears to be published regarding its crystalline form. It is stated, however, in Graham's 'Chemistry,' vol. 1, that the most concentrated acid, when frozen, often yields regular six-sided prisms of a tabular form. Chaptal describes the crystals as being six-sided prisms terminating in pyramids with six faces. Both accounts agree in placing them in the hexagonal system. In order to see if the appearance of the crystals agreed with either of the above descriptions, some sulphuric acid was strengthened by boiling for some time, and then cooled down until it froze. The crystals obtained were found to be long six-sided prisms ending in pyramids, as described by Chaptal, and no prisms of a tabular form were observed. It is thus interesting to find that both anhydrous sulphuric and selenic acid crystallise in prisms in the hexagonal system, but it remains doubtful whether or not they are strictly isomorphous.

Selenic acid in the anhydrous condition possesses a powerful affinity for water, absorbing it quickly from the atmosphere. Their combination is attended with contraction and considerable evolution of heat, but less so than in the case of water and sulphuric acid. Like the latter, it disintegrates and blackens many organic substances, such as cork, india-rubber, &c. From others it withdraws the elements of water; thus, alcohol heated with it yields ethylene, and glycerine, acrolein. On cellulose it has an action similar to that of strong sulphuric acid, paper being converted by it into a tough parchment-like substance. For this reason it should not be filtered through filtering paper, except when cold and very dilute. Iodine dissolves in the superfused acid when heated, forming a brown-coloured solution. It is acted on violently by pentachloride of phosphorus in the cold—a reaction which we are at present examining. Oxychloride of

phosphorus also acts strongly upon it when warmed slightly, the reaction being attended with copious evolution of gas and reduction apparently to lower compounds. Selenium dioxide dissolves in it when heated, but the greater part crystallises out again in the cold. There is no evidence of formation in this way of an acid analogous to hyposulphuric acid, $H_2S_2O_6$. The crystals of the solid acid dissolve in strong sulphuric acid, and also in Nordhausen acid.

The specific gravity of the superfused acid, taken with a Sprengel tube at 15°, was found to be 2·6083. The specific gravity of the solid acid was taken in pure benzene of specific gravity 0·8851, which is not acted upon by it in the cold, and in which it is insoluble. As might be expected, it at once blackens commercial benzene. Its specific gravity, taken in this way, proved to be 2·9508 at 15°. It thus resembles anhydrous sulphuric acid in being denser in the solid than in the liquid state. The specific gravity of the liquid acid is much greater than that of anhydrous sulphuric acid, 1·8384; and on the other hand the specific gravity of the solid acid is less than that of anhydrous telluric acid, which is stated by F. W. Clark to be 3·425 at 18·8° ('American Journal,' vol. 14, 1877, p. 281; vol. 16, 1878, p. 401).

Monohydrated Selenic Acid: its Preparation and Properties.

Some selenic acid, which had been concentrated on the water-bath, was heated for some time in a vacuum at 100°, and its strength determined: 0·7858 gram neutralised 20·00 c.c. of seminormal soda solution, equivalent to 92·08 per cent. of selenic acid. The acid so prepared was diluted with sufficient water to reduce its strength to 88·96 per cent., corresponding to a monohydrated acid, H_2SeO_4,H_2O. It was then poured into a wide tube and its temperature gradually lowered, the same precautions being taken as to stirring and admission of air as were adopted previously in freezing out the anhydrous acid. Its viscosity increased with the fall in temperature, until at −32° it froze into a mass of crystals. These were melted and re-crystallised several times, and the resulting product examined.

A few of the crystals obtained were long needles, but most of them were large and broad, having a general aspect to which the term "glacial" might be applied appropriately, but differing in appearance under the microscope from those of glacial sulphuric acid.

Its melting point was found to be 25°. Like glacial sulphuric acid, and also like anhydrous sulphuric and anhydrous selenic acid, when once melted it exhibits the property of superfusion, and to as great an extent as the last-mentioned acid, since it may be cooled to more than 50° below its melting point, with constant stirring, before it again freezes. When frozen it remains quite solid at ordinary temperatures; but if the bottle containing it be removed to a warm

room, or much handled, it commences to melt. Like the anhydrous acid, it at once freezes at any temperature below its melting point when a crystal of the same acid is dropped into it. It resembles the anhydrous acid also in having a melting point much higher than sulphuric acid of the same strength, that of glacial sulphuric acid being given by Pierre and Puchot as 7·5°, by Jacquelin as 8°, and by Marignac as 8·5°. It resembles the latter acid in having a melting point lower than its anhydrous acid, but while the difference is about 2·5° in the case of sulphuric acid, the melting points of the two selenic acids differ by 33°. In the following table their melting points are compared:—

	Anhydrous.	Monohydrated.
Sulphuric acid..........	10·5°	8°
Selenic acid............	58·0	25

It may be well to state here that 0° is given erroneously in several chemical works as the melting point of anhydrous sulphuric acid. Marignac, the most recent investigator who has studied the subject, assigns the temperature 10·5° as its true melting point.

The superfused monohydrated selenic acid has a specific gravity of 2·3557 at 15°. That of the solid acid was taken in pure benzene, in which it is insoluble, and on which it is without action at ordinary temperatures, even after standing all night. A portion of the liquid acid was poured into the specific gravity bottle, a crystal dropped into it, and the acid, having become firm and cold, weighed, and the bottle filled with benzene. The specific gravity was found to be 2·6273 at 15°. It thus resembles the anhydrous acid in being denser in the solid than in the liquid state; while melting, the crystals sink rapidly in the liquid portion.

This acid commences to boil at 205°, the acid vapour given off being at first very weak, but it increases in strength with the rise in temperature. A more dilute acid gives off water only until the temperature reaches 205°. Dilute sulphuric acid is stated to behave in a similar manner, giving off nothing but water until the boiling point reaches 205—210°, at which temperature it has the strength of the monohydrated acid, H_2SO_4,H_2O.

The ease with which an acid of this strength can be obtained and crystallised, supplies a means of separating impurities from selenic acid, all that is necessary to do being to boil a dilute acid until the temperature reaches 205°, cool, and drop in a crystal from an acid already frozen; the resulting crystals can then be melted and recrystallised.

It may be well here to draw attention to the conflicting statements which are made in various works regarding the crystalline form of glacial sulphuric acid. Watts's 'Dictionary,' vol. 5, and Richter's

'Inorganic Chemistry,' state that it crystallises in six-sided prisms; but in several chemical works the crystals are described as rhombic prisms, while Pelouze and Fremy mention that it forms large transparent crystals which are rhomboidal prisms. The crystalline form is thus referred to three different systems. The first description is probably copied by mistake from that of the anhydrous acid. The most recent investigators of the point are Jacquelain ('Annales de Chimie,' vol. 30, 1850, p. 343), and Pierre and Puchot ('Annales de Chimie,' vol. 2, 1874, p. 164). The latter say that the crystalline form appeared to them to be the oblique rhomboidal prism, and that they obtained the crystals, some very large, others thin and very long. Jacquelain describes them as being oblique prisms very inclined and very large; and states that he obtained them, by a rather slow crystallisation, distinctly oblique and very short, and by a quick crystallisation, in very long oblique prisms.

The Existence of higher Hydrates.

A portion of dilute selenic acid was concentrated on the water-bath and its strength taken: 0·8603 gram neutralised 19·48 c.c. of seminormal soda solution, equivalent to 81·92 per cent. of selenic acid. The acid so prepared was diluted with sufficient water to reduce its strength to 80·11 per cent., corresponding to a dihydrated acid, $H_2SeO_4,2H_2O$, and its temperature then gradually lowered. When kept at $-51°$ for some time it became as viscous as thick syrup, but did not freeze. The last acid was then diluted to a strength of 57·32 per cent. of selenic acid, corresponding to a hydrate of the composition $H_2SeO_4,6H_2O$. The acid thus prepared did not freeze when kept at $-49°$, and was quite liquid at that temperature.

Although no proof of the existence of higher hydrates than the monohydrated acid was obtained in the foregoing experiments, it appears probable that a dihydrated acid, and perhaps other hydrates, are capable of existing. Sulphuric and telluric acid have both been obtained as dihydrates. Considerable heat is evolved when monohydrated selenic acid is mixed with sufficient water to reduce its strength to that of a dihydrated acid. When the latter is further diluted, there is an additional slight evolution of heat. It is probable that as the freezing point of monohydrated selenic acid is considerably below that of the anhydrous acid, so the freezing point of a dihydrated acid is still lower. In order to get an approximate idea of the amount of water which anhydrous selenic acid absorbs, a portion weighing 0·9776 gram was placed on a watch-glass protected from dust, but with free access of air. After twenty-four hours the acid weighed 2·0284 grams, showing an absorption in that time of between 8 and 9 molecules of water by 1 molecule of anhydrous acid. Another portion weighing 0·4416 gram was exposed until it ceased to

absorb water; the acid then weighed 1·8152 grams, indicating absorption of rather more than 25 molecules of water by 1 molecule of anhydrous acid. Having arrived at this stage, it commenced to give off a little of the water which it had previously taken up, but the weather became warmer just at this period, so that probably the above amount does not represent the total absorption of which the acid is capable. It is less, however, than the amount taken up by the molecule of sulphuric acid, which is variously stated at from 80 to 100 molecules of water.

The Conditions which affect the Freezing Points of Selenic Acid and Sulphuric Acid.

It has long been recognised that in order to determine the melting point of a chemical compound with accuracy it is necessary, by crystallisation or other means, to obtain it in a pure condition. The necessity for such a precaution is well shown in the case of the oxides and acids of sulphur and selenium. An example taken from the former class of bodies is furnished by sulphuric anhydride. Up to a comparatively recent date great diversity of opinion prevailed regarding the melting point of this substance until Weber showed that, as hitherto examined, it had usually contained a minute quantity of water, which had the effect of altering its melting point, crystalline form, and other properties. Sulphuric acid supplies another instance of a similar effect. Its melting point in the anhydrous condition is 10·5°, while that of the monohydrated acid is 8°; yet commercial sulphuric acid has usually been found to remain liquid above a temperature of −30° or −40°, and it is stated in some chemical works that by addition of a little water to the commercial acid its freezing point has been lowered to −80°; but it is not mentioned whether or not this occurred in closed or open vessels. A still more striking example of the influence of want of purity upon the melting point is afforded by selenic acid. In the anhydrous state it melts at 58°, but a slightly dilute acid, as we have found, was frozen only when a temperature of −51·5° was reached, showing a fall of 109·5°, and probably further dilution would be attended by a still greater reduction of the freezing point. The depression of the freezing point can be due only partly to superfusion since the superfused anhydrous acid freezes at about 5°. The monohydrate present in the dilute acid therefore exerts an influence in lowering the freezing point of the anhydrous acid, and also its melting point. An analogous action probably occurs in the case of some metallic salts, which, although without apparent chemical action upon each other, have a lower fusing point when mixed than when heated separately. A consideration of the foregoing facts leads to the conclusion that *dilute selenic acid having a strength greater than* 88·96 *per cent. contains the*

Table II.

Percentage of anhydrous selenic acid.	Specific gravity.
99·73	2·6083
99·50	2·6051
99·00	2·5975
98·50	2·5863
98·00	2·5767
97·50	2·5695
97·00	2·5601
96·00	2·5388
95·00	2·5163
94·00	2·4925
93·00	2·4596
92·00	2·4322
91·00	2·4081
90·00	2·3848
89·00	2·3568
88·00	2·3291
87·00	2·3061
86·00	2·2795
85·00	2·2558
84·00	2·2258
83·00	2·1946
82·00	2·1757
81·00	2·1479
80·00	2·1216
79·00	2·0922
73·50	1·9675

The rate of increase of specific gravity is not uniform for equal increments of strength. It diminishes as the strength increases, as in the case of sulphuric acid, but not regularly. The diminution is very marked at the highest strengths. When sulphuric acid has arrived at the greatest strength attainable by ebullition—98·66 per cent.—its specific gravity is stated to decrease until the anhydrous acid, H_2SO_4, is reached. Selenic acid behaves dissimilarly in this respect; the increase of its specific gravity, although not uniform, is maintained throughout.

Berzelius ('Traité de Chimie,' 1830) mentions that selenic acid of 95·9 per cent. strength has a specific gravity of 2·6. Fabian gives 2·609 as the specific gravity of an acid of 94·9 per cent. strength, and 2·627 for an acid of 97·4 per cent. It will be seen that these results do not agree, nor are they consistent with those we have obtained. If values for the strengths mentioned be calculated from the foregoing

tables, an acid of 95·9 per cent. will be found to have a specific gravity of 2·5366, while the specific gravities of 94·9 per cent. and 97·4 per cent. acids will be respectively 2·5141 and 2·5680, being less than those assigned above. Furthermore, the difference between the two specific gravities given by Fabian is much less than we find between two acids differing by 2·5 per cent. The most probable explanation of the discrepancy is that the acids which gave the above results contained sufficient selenium dioxide to raise their specific gravities appreciably. It may be shown that the effect of the development of selenium dioxide in selenic acid is to increase the specific gravity relatively to the acidity. In the acids above-mentioned, selenium dioxide would exist as such, and not as selenious acid, owing to dissociation of the latter at a temperature below those at which the acids were formed, and the weak affinity for water possessed by the resulting dioxide. Clausnizer ('Liebig's Annalen,' vol. 196, 1879, p. 265) gives the specific gravity of selenium dioxide at 15·3° as 3·9538. We have also recently taken its specific gravity, and are in a position to confirm his result. It is thus more than one and a half times as dense as the strongest selenic acid. C. Blarez ('Comptes Rendus,' vol. 103, 1886, pp. 804—806) has examined the saturating power of selenious acid. He finds that it is monobasic with cochineal or methyl-orange. With litmus, it is monobasic to ammonia, lime, strontia, and baryta, but with soda or potash the litmus only becomes blue-violet when about 1·5 equivalent of alkali is added. We have obtained a like result with soda or potash and litmus. When one equivalent of acid is saturated, there is a distinct change in the colour of the litmus, so that in the absence of other acids it might be used as an indicator for selenious acid. Taking 1·5 equivalent of alkali as the limit, the molecule of selenium dioxide in solution will have a less saturating power than that of selenic acid in the ratio of 2 : 1·5. This will be partly counterbalanced by the higher molecular weight of the latter, but the final effect of the substitution of selenium dioxide for selenic acid will be to reduce the acidity. A gravimetric method, by which selenium dioxide would be oxidised and estimated as selenic acid, would also show a less acidity compared with the specific gravity than if the pure acid were used, but not to the same extent as when a volumetric process is employed for estimating the strength.

The Action of Heat upon Selenic Acid.

Action of Heat in a Vacuum.—The effect of heating dilute selenic acid in a vacuum up to 180° has already been described—dilute acid distils until that temperature is reached, when the anhydrous acid remains.

The result of further heating is merely for a time to raise the

temperature; the acid does not distil in the anhydrous condition. At about 200° it begins to decompose slowly, and at higher temperatures rapidly, into selenium dioxide, oxygen, and water. The latter serves to dilute a portion of the remaining acid, which then at once distils. In order to examine the effect of distilling the anhydrous acid destructively in a vacuum, a portion was heated in a flask connected with a condensing arrangement until rapid decomposition took place. The residue always consisted of a mixture of anhydrous selenic acid with selenium dioxide, the proportion of the latter increasing with the rise of temperature and length of time of heating. The former was proved to be present by dropping in a crystal of anhydrous acid, when the liquid froze, and it also gave a coloration in the cold with the selenium test for the same acid which will be described hereafter. A portion which had been distilled for some time left a residue of which 0·7107 gram neutralised 18·50 c.c. of seminormal soda solution, using litmus as indicator, equivalent to 94·27 per cent. of acid calculated as selenic—a result which might be expected, owing to the influence of the selenium dioxide in diminishing the acidity, as has been already pointed out. The distillate consisted of selenium dioxide mixed with selenic acid. The latter was in a dilute state, since it would not solidify on addition of a crystal of anhydrous acid, nor would it respond to the selenium test: 0·4703 gram neutralised 11·11 c.c. of seminormal soda solution, equivalent to 85·46 per cent. of acid calculated as selenic. Taking into account the diminution of acidity caused by the selenium dioxide, it is evident that a very concentrated, although not anhydrous, acid distils over.

Action of Heat under ordinary Pressures.—When dilute selenic acid is boiled at ordinary pressures nothing but water is evolved until 205° is reached, at which temperature it has the composition of the monohydrated acid. In these respects it behaves like dilute sulphuric acid. After passing 205° the distillate contains at first mere traces of selenic acid, but its strength gradually increases. A portion of acid distilled between 205° and 227° yielded a distillate of which 0·6826 gram neutralised 0·10 c.c. of seminormal soda solution, equivalent to 0·53 per cent. of selenic acid. On further heating from 227° to 260° a distillate was obtained, of which 0·3818 gram neutralised 0·32 c.c. of seminormal soda solution, equivalent to 3·03 per cent. of selenic acid. At the latter temperature the acid commenced to distil over in white fumes. 0·6878 gram of the residue left at 260° neutralised 17·76 c.c. of seminormal soda solution, equivalent to 93·41 per cent. of selenic acid. At higher temperatures a portion of the acid is decomposed, the distillate being kept weak by the water continuously set free in the decomposition; and at still higher temperatures much selenium dioxide also distils over. A portion of acid

which commenced to boil at 235° was heated until the temperature rose to 325°: 2·1542 grams of the distillate neutralised 3 c.c. of seminormal soda solution, equivalent to 5·04 per cent. of selenic acid. The residue was then further heated until the greater part of the acid was decomposed and the strength of the distillate taken: 1·6176 grams neutralised 16·5 c.c. of seminormal soda solution, equivalent to 36·90 per cent. of acid calculated as selenic, but much of the acidity was due to selenious acid, a large amount of which was present in the distillate. It is thus seen that the acid which distils under ordinary pressures is always highly dilute.

When anhydrous selenic acid is strongly heated under ordinary pressures a portion of it is decomposed into selenium dioxide, oxygen, and water, the latter serving to dilute the remaining acid. It will then no longer solidify on addition of a crystal of the anhydrous acid. After further heating it becomes sufficiently dilute to distil over in the manner already described. It is instructive to compare with this the action which takes place in the case of anhydrous sulphuric acid. Marignac found ('Annales de Chimie,' vol. 39, 1853, p. 184), that the latter when heated gives off sulphuric anhydride until the remaining acid is reduced to the same strength as that yielded by a more dilute acid on prolonged ebullition—about 98·66 per cent. In the case of selenic acid the anhydride is evidently unable to exist free at elevated temperatures, but breaks up into selenium dioxide and oxygen.

It is worthy of note that a remarkable coincidence exists between the temperature at which selenic acid is decomposed and that at which selenium dioxide sublimes. The latter is not stated very definitely in any of the chemical text-books; we have found, however, that at 280° rapid sublimation takes place, while at 250° volatilisation proceeds slowly. When selenic acid is kept for some time at the latter temperature it is always found to contain traces of selenium dioxide, while at 280° decomposition proceeds with rapidity. It is probable that a connexion exists to a considerable extent between the phenomena, and that the explanation of the coincidence may be that the comparatively weak affinities which bind the molecule of selenic acid are unable to prevent its disruption when once a temperature is reached at which the tendency of its constituents to separate is aided by the expansion attending a change to the gaseous condition. We have seen that both phenomena are affected by the temperature; they are also dependent to some extent upon the pressure. Anhydrous selenic acid commences to dissociate in a vacuum at about 200°. We found that when selenious acid was heated under similar circumstances water was quickly evolved, and after prolonged heating of the residue a distinct white ring of selenium dioxide was deposited on the cold portion of the neck of the flask.

The Action of Sulphur upon Selenic Acid.

When powdered sulphur is mixed with liquid anhydrous selenic acid there is no action in the cold, but if the mixture is heated to 63° the sulphur dissolves in the acid with production of an exceedingly deep indigo-blue colour. There is some action, but slight, at 58°. The body which is produced is very unstable, since it begins to decompose at the temperature at which it is formed, with evolution of sulphur dioxide, and reduction of the selenic acid to selenious acid. If water is added when the colour has first developed, sulphur is deposited in soft yellow flakes; but after decomposition has commenced, the addition of water throws down red selenium chiefly, owing to the reaction between the sulphurous and selenious acids which are formed.

A similar action has long been known to occur between sulphur and sulphuric acid. In 1804 Bucholz (Gehlen's 'Neues Journal der Chemie,' vol. 3, p. 7) discovered that sulphur dissolved in the latter acid with the formation of a blue colour, the sulphur being re-precipitated on addition of water. This reaction was subsequently investigated by Vogel, Schweigger, Berzelius, Wach, and Stein, and in 1875 Weber succeeded in isolating the blue compound (Poggendorff's 'Annalen,' vol. 156, p. 531), and discovered it to be a sesquioxide of sulphur, S_2O_3, but very unstable, decomposing at ordinary temperatures with evolution of sulphur dioxide.

The Action of Selenium upon Selenic Acid.

A reaction takes place between selenium and anhydrous selenic acid in the cold. The selenium dissolves in the acid with production of an intense and beautiful green colour. The presence of a minute quantity of water prevents the colour from developing fully until the acid is warmed. The acid is capable of dissolving a considerable quantity of selenium. If kept in a closed vessel the colour thus produced is very permanent, being unaffected for months during summer weather, but if the acid be heated to 75° the colour disappears, and the selenic acid is found to be partly reduced to selenious acid. Addition of water, when the colour has developed, throws down a voluminous red precipitate of selenium; and exposure to the air in an open vessel for a few minutes is sufficient, from the same cause, to change the green colour to red. It will be shown hereafter that this green colour is probably caused by the formation of a new oxide of selenium. Its production with the anhydrous acid affords a test for the latter, which we have already made use of in the examination of the effect of heat upon selenic acid.

In 1827 Magnus found that selenium was dissolved by strong sulphuric acid, with development of a green colour, and was re-precipitated on addition of water. Berzelius and Fischer subsequently

investigated the reaction, and in 1875 Weber (Poggendorff's 'Annalen,' vol. 156, p. 545) isolated the green compound, and found it to be a sulphoxide of selenium, $SeSO_3$. The colour given by selenium with sulphuric acid is a dark green, not so bright as that which it produces with selenic acid.

The Action of Tellurium upon Selenic Acid.

Like selenium, tellurium reacts upon anhydrous selenic acid in the cold, but with production of a purple-red colour. Even when the acid is not perfectly anhydrous, it will still give a coloration in the cold. The compound thus formed is probably very unstable, since the colour disappears at so low a temperature as 19°, the selenic acid being partly reduced to selenious acid. If water is added when the colour has developed, tellurium is thrown down in sooty flakes, and exposure to moist air produces the same effect.

Tellurium reacts in a similar manner with strong sulphuric acid, with development of a red colour, and re-precipitation on addition of water. This was first observed by Müller von Reichenstein. In 1789 the same reaction was investigated by Klaproth, and afterwards by Magnus and Fischer. The red compound was isolated by Weber in 1882 ('Journal für Praktische Chemie,' vol. 25, p. 218), and soon afterwards independently by Divers and Shimosé ('Journal of the Chemical Society (Transactions),' vol. 43, p. 319), and was ascertained to be tellurium sulphoxide, $TeSO_3$.

Comparison of Sulphoxides with Selenoxides.

The coloured compounds first formed by the action of sulphur, selenium, and tellurium upon sulphuric acid were subsequently produced by the action of the same bodies upon sulphuric anhydride, and were isolated by forming them in the latter manner, and then removing the excess of anhydride. As has been already mentioned, they were found to be sulphoxides having the composition respectively SSO_3, $SeSO_3$, and $TeSO_3$. Considering the similarity of the reactions which produce them, and of the decompositions to which they are liable, it is very probable that the coloured bodies formed with selenic acid are selenoxides, analogous to the sulphoxides already known, the sulphur in the latter being replaced by selenium. A comparison of the formulæ of the two series of bodies as represented in the following table, leads to some interesting considerations :—

Colour of compound.	Sulphoxides.	Selenoxides.
Blue	SSO_3	$SSeO_3$
Green	$SeSO_3$	$SeSeO_3$
Red	$TeSO_3$	$TeSeO_3$

The green body formed by the action of selenium upon selenic acid is, in all probability, a new oxide of selenium, having the composition Se_2O_3, analogous to the sesquioxide of sulphur formed by the action of sulphur upon sulphuric acid.

The colour of the above compounds appears to be almost entirely due to the element which is added on to the anhydride; the sulphur or selenium in the residual portion of the molecule has scarcely any effect upon the colour.

The green body $SeSO_3$ must be isomeric with the blue compound $SSeO_3$, and consequently the atoms of sulphur and selenium in each of these bodies must occupy dissimilar positions in the molecule.

The Possible Existence of Selenic Anhydride.

A consideration of the foregoing facts leads to the conclusion that selenic anhydride, which has not up to the present time been obtained, is yet capable of existing in a free condition. Just as the sulphoxides are direct addition compounds of sulphur, selenium, and tellurium respectively, with sulphuric anhydride, so the selenoxides must be regarded as addition compounds of the elements mentioned, with selenic anhydride. It is difficult to conceive that bodies so constituted are capable of existing free, and that the anhydride is unable to do so. On the other hand, it is probable that it dissociates at a comparatively low temperature, but one that is higher than 75°—the highest temperature at which the most stable of the selenoxides has been as yet found to exist. Selenious anhydride is always obtained, and not selenic, under similar conditions to those which produce sulphuric anhydride, and involving a high temperature.

We have examined the action of heat upon various selenates. Those selected for the purpose were the selenates of antimony, bismuth, platinum, lead, and silver, and also ferric and mercuric selenates. In no instance was selenic anhydride obtained, but selenious anhydride was evolved in all cases when the above selenates were strongly heated.

Von Gerichten ('Liebig's Annalen,' vol. 168, 1873, p. 214) endeavoured to form selenic anhydride in a similar manner to that by which sulphuric anhydride is usually obtained—by leading a mixture of oxygen and the vapour of selenious anhydride through red-hot platinum sponge. He states that in one experiment a white deposit was obtained which consisted partly of selenious anhydride, but probably also contained selenic anhydride, since it dissolved in water with a hissing sound, and the solution was found to contain selenic acid. In subsequent experiments a decidedly negative result was obtained, since only selenious anhydride was found in the product.

In order to examine if selenic anhydride is produced in the manner just described, the following experiments were made:—

Some platinum sponge was introduced into a combustion tube, and a quantity of selenious anhydride placed behind it. The platinum sponge was kept at a red heat, and the selenious anhydride was also heated until it sublimed. Oxygen, which was first dried by passing through a series of tubes containing calcium chloride and sulphuric acid, was led through the platinum sponge, carrying the vapour of the selenious anhydride with it. A white deposit formed in front of the platinum sponge, which, on examination, was found to consist solely of selenious anhydride.

The experiment was repeated, substituting selenium for selenious anhydride. The vapour of the selenium, on passing through the platinum sponge, was oxidised to selenious anhydride, but no selenic anhydride was formed.

The next experiment was similar to the first, but platinum black was substituted for platinum sponge. As before, only selenious anhydride was deposited.

In the next experiment selenious anhydride was mixed with platinum sponge, and placed within a tube arranged in such a way that it could be placed in an oil-bath. The temperature was then raised gradually to 250°, oxygen being led through continuously. At the temperature mentioned a sublimate formed, and this, as well as the platinum sponge, was treated with water and tested, but no selenic acid was found.

The next experiment was similar to the last, except that the selenious anhydride was mixed with platinum black instead of platinum sponge, but, as before, nothing but selenious anhydride was obtained.

As may be seen from the above experiments, we have been unable to confirm the observation made by von Gerichten.

We have found that oxygen in the active condition is unable to oxidise selenious anhydride. Oxygen, which was first passed through a series of drying tubes, was ozonised by means of a Houzeau's ozoniser, and then led slowly through a tube containing selenious anhydride for three hours and a half. At the end of that time the tube was disconnected, the ozone displaced by a current of dried air, and the contents of the tube dissolved in water and tested for selenic acid, but no trace of it was detected.

The Selenates of Antimony, Bismuth, and Platinum.

In the examination of the action of heat upon selenates, the three above mentioned were included. We have been unable to find any published account of these, and accordingly give here a short description of them.

Antimonious Selenate.—This body was formed by heating metallic

antimony with selenic acid until it dissolved, and then continuing the heating until the excess of acid was driven off. It forms a white crystalline mass, which under the microscope is seen to consist of minute prisms. It is not very soluble in acids, but on boiling with selenic acid for some time it dissolves slightly, but apparently without formation of an acid salt, as the crystals deposited on evaporating the solution have the same shape and size as the original salt. It is not decomposed on boiling with water, nor is it soluble; if, however, the excess of selenic acid has not been expelled in its preparation, a little of it dissolves in the weak acid formed, and crystallises out again on cooling.

Bismuthous Selenate.—This was prepared by boiling bismuthous carbonate with selenic acid, and continuing the heating until the excess of acid was expelled. It crystallises in minute white prisms. It is insoluble in water, and is not decomposed by it on boiling, but if selenic acid is present it dissolves. It is also soluble in sulphuric acid on boiling, and readily in hydrochloric and nitric acid. Potash and soda decompose it, uniting with the selenic acid.

Platinic Selenate.—This was prepared by heating platinic chloride with selenic acid until the hydrochloric and excess of selenic acid were expelled. It is a dark-brown body, which dissolves slightly in water on boiling, forming a yellowish-brown solution. It is insoluble in alcohol. It dissolves readily in hydrochloric acid, and leaves metallic platinum on ignition.

The Action of Phosphoric Anhydride upon Selenic Acid.

When sulphuric acid is heated strongly with phosphoric anhydride sulphuric anhydride is evolved; on the other hand, the effect of heating a mixture of anhydrous selenic acid and phosphoric anhydride to a high temperature is, that selenious anhydride is given off, and not selenic. In order to see if a different reaction occurs between the latter two bodies at a lower temperature, some anhydrous acid was mixed with phosphoric anhydride in a flask, which was then tightly closed and placed in the water-oven. After heating for some time a clear solution was obtained, and the flask was then removed and allowed to stand in the cold. Crystals were slowly deposited, which under the microscope were seen to be transparent cubes. The acid liquid was drained off, and the crystals examined. So far as an analysis could be carried out the result agrees with the view that the body thus obtained is selenic anhydride, SeO_3, but the quantity of material at our disposal at the time was too small to admit of its satisfactory separation and examination. We are, however, at present engaged in operating upon larger quantities, and hope, at no distant date, to submit our results to the Royal Society.

Selenium Oxychloride.

It may be useful here to mention a reaction, hitherto undescribed, by which selenium oxychloride can be obtained. It consists in distilling a mixture of sodium chloride and selenium dioxide. The reaction is a simple one—

$$2SeO_2 + 2NaCl = Na_2SeO_3 + SeOCl_2.$$

Half the selenium remains as sodium selenite.

III. "On the Wave-length of the chief Fluting seen in the Spectrum of Manganese." By J. NORMAN LOCKYER, F.R.S. Received April 6, 1889.

In a paper communicated to the Royal Society in November, 1887, I pointed out that in the spectra of certain classes of heavenly bodies there are several lines and flutings which occur in the low-temperature spectra of meteorites.

In a subsequent paper* I gave tables showing further that lines occupying nearly the same position had also been recorded in the spectra of auroræ. The coincidences were really surprising, and I therefore suggested that the spectrum of the aurora might in part be due to particles of meteoric dust in the upper parts of our atmosphere.

One of the most constant flutings thus apparently common to the spectra of meteorites, meteor-swarms (nebulæ and stars as well as comets), and auroræ is one near wave-length 558. I, therefore, suggested that the remnant of this fluting, which is seen in the spectrum of manganese (whether due to the metal or a compound is unimportant in the present connexion), was the origin of the chief line of the aurora spectrum. I distinctly stated that only small dispersion had been employed of set purpose, and that the wave-lengths given had no claim to great accuracy.

From a detailed discussion of all the published observations available to me,† the suggestion as to the meteoritic nature of the aurora seemed fully justified, both as regards the general characteristics of the spectra and the positions of the lines and flutings observed, but I was particularly careful to point out that the object of my paper was mainly to direct further inquiries.

Dr. Huggins has communicated an interesting paper to the Royal Society,‡ in which he gives the results of some very careful measure-

* 'Roy. Soc. Proc.,' vol. 43, p. 320.
† 'Roy. Soc. Proc.,' vol. 45, p. 217.
‡ 'Roy. Soc. Proc.,' vol. 45, p. 430.

ments of the chief aurora line which he made with considerable dispersion in 1874, but which have never before been published. As the position determined by him differs from the approximate wave-length of the manganese fluting, which, so far as I know, had only been previously given by Lecoq de Boisbaudran,* I have deemed it desirable to redetermine the wave-length with increased dispersion. For this purpose a 4-prism Steinheil spectroscope was employed, the wave-lengths being determined by comparison with the solar spectrum and Ångström's map.

Manganese (Lockyer).

Sodium (Ångström).

Aurora (Huggins).

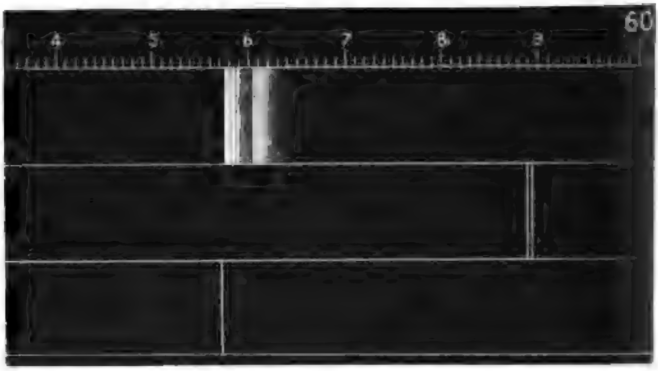


FIG. 1.—Diagram comparing the manganese fluting with the aurora line, the D lines being also shown for comparison.

The result of the observations with increased dispersion, which were made by my assistant, Mr. Fowler, is shown in the diagram, from which it will be seen that the wave-length of the most refrangible edge of the first fluting which is not perfectly sharp, is about 5576. The wave-lengths of all the details of the fluting shown are—

Mn (1) ..... { 5576 Most refrangible edge.
5583 } Double maximum.
5589 }
5604 Another maximum.

Taking the mean of the values of the aurora line quoted by Dr. Huggins, viz.:—

Vogel, 1872	5571·3 ± 0·92
Huggins, 1874	5571·0 ± 0·5
Gyllenskiöld's mean, 1867–1884 ..	5570·0 ± 0·88

we get 5570·76 ± 0·76. This gives the limiting values as 5571·5 and 5570·0.

* 'Spectres Lumineux,' p. 116.

The distances of these from the most refrangible edge of Mn (1) are respectively 0004·5 and 0006·0.

If we take the maximum divergence from the Mn fluting, viz., 0006, it is equal to the distance between the D lines of sodium, which are also shown in the diagram for purposes of comparison. The divergence of the other extreme, viz., 5571·5, is about two-thirds of the distance between the D lines.

Putting Dr. Huggins's observations aside, as having been made with considerable dispersion, any observer who is at all acquainted with the small dispersion generally necessarily employed in observations of the aurora, will quite understand that it is almost impossible to differentiate to these small amounts with such dispersion. The aurora spectrum being usually faint, it is necessary to employ a wide slit, so that the chances of seeing D double are very small.

Dr. Huggins states that in his observations "the reading showed the line to fall about midway between two strong lines in the spectrum of tin, λ 5564 and 5587 respectively." The wave-lengths given by Thalèn for these lines of tin are 5565 and 5588, so that a line exactly half-way between them would have a wave-length of very nearly 5575·5 (whether we take the wave-lengths given by Huggins or by Thalèn), which almost absolutely corresponds to the Mn fluting. The value deduced, however, was 5571, which would place the line not half-way between the tin lines, but less than one-third of the distance between them from the most refrangible one. If the statement that the line fell midway between the tin lines is to be relied on, it seems probable that there was an accidental shift before the observations of the tin spectrum were made, but the details of the comparison not having been recorded, it is impossible to say with certainty whether there was any such shift or not. Such a shift would also affect the results of the tellurium comparison referred to by Dr. Huggins to the same extent as that of the tin comparison, since the aurora line was not again brought on the cross-wires. The value determined by Dr. Huggins from a comparison with the iron lines in the neighbourhood was 5571·5, which is a little higher than that derived from the tin and tellurium comparisons.

[*Note, May 7th.*—I have received a note from Dr. Huggins referring to the above paragraph, in which I make a quotation from his paper, and suggest a possible source of error. I have his permission to publish the following extracts from his letter :—

"I thought it would be clear that the statement that the reading showed the line to fall between the lines of tin (or 'about midway between'), was nothing more than a rough indication to me what metal to take, and that *no importance was attached* by me to this *quite rough* determination of the metal to be chosen.

"I find, on reference to my note-book, that the words written at the time are 'map showed reading between lines of tin.' It was in expanding this sentence for my paper that the words 'about midway' came in. I noticed when the paper was in proof that the expression was not strictly accurate, but I allowed it to pass, as the sentence was obviously merely a rough indication of position for the selection of the metal for comparison. There is absolutely no ground for the suggestion of any shift, as the position of the aurora line from the direct comparison with tin agrees with the former one (No. I)."

From the above, it appears that the statement that the aurora line fell "about midway between" the lines of tin was not strictly accurate, and this consequently led to a misunderstanding].

The importance of actually confronting the spectrum of the aurora with that of manganese, and definitely determining the coincidence or non-coincidence of the brightest fluting of manganese with the chief aurora line is therefore obvious, and no opportunity of making the observations should be lost.

In support of the suggestion that the chief aurora line occupied the same place in the spectrum as the brightest fluting of manganese, I quoted the observations of Smyth and Proctor, which placed the line considerably less refrangible than Ångström's observation (5567). I attached no special weight to these observations beyond the fact that I knew the observers to be absolutely trustworthy, and there can now be no doubt that they were justified in differing from Ångström. Gyllenskiöld's statement of Proctor's value ($5595 \pm 25{\cdot}0$) was based upon a curve constructed from the data contained in a small sketch of the spectrum with an empirical scale given in 'Nature' (vol. 3, p. 346), and is, therefore, not so important as Proctor's own statement: "My own measures give me a wave-length very slightly greater than those of Winlock and Ångström."*

Winlock's value being 557, it seems pretty certain that the line is decidedly less refrangible than 557. Gyllenskiöld estimated the probable error of Proctor's value as equal to one division of the scale (the distance between C and F being about 96 divisions) or more than four times the distance between the D lines.

With reference to Krafft's observations at Bossekop in Alten, Dr. Huggins says: "I have already pointed out that Krafft's measures were not made under circumstances which assured to them a high degree of accuracy, and Krafft's own words, which I have quoted, disclaim expressly any special attempt on his part to redetermine the position of the principal line with a higher degree of accuracy than the observers who preceded him." Dr. Huggins does not appear to

* 'Nature,' vol, 3, p. 384.

have been sufficiently impressed by the modesty of Krafft's statement with respect to the accuracy of his observations.

Although his attention was directed mainly to other observations, and spectroscopic observations were of secondary importance, the instruments were specially designed for the work, and it is fair to assume that the measurements were made as carefully as possible. Krafft's statement—"Leider gestatteten die obligatorischen Beobachtungen nicht, den spectroscopischen Untersuchungen die gehörige Aufmerksamkeit angedeihen zu lassen. . . . . Ich glaubte ausserdem diese Messungen um so mehr auslassen zu können, als der Platz der gewöhnlichen Nordlichtlinie oft und sehr genau bestimmt ist"—does not, I think, necessarily imply that the observations actually made were made carelessly.

Further inquiry is, therefore, I think, necessary before we can finally conclude that the suggestion that the chief line of the aurora is the remnant of the manganese fluting at 558 is inadmissible, as Dr. Huggins states it to be.

That the spectrum of the aurora may be due to the integration of two or more elementary spectra, as first suggested by Vogel in 1871, is quite in accordance with the meteoric dust theory. As I stated in my former paper,* "the aurora spectrum can be built up from the lowest temperature spectra of manganese, magnesium, lead, and thallium, and the brightest flutings of carbon." And not only this, but when a line of any substance appears alone, it is generally the line which first appears in its spectrum in the laboratory; when two appear, the second line is also generally the one which is added to the first by the first increase of temperature. Any departure from this may be ascribed to the incompleteness of the observations. The differences between the spectra of auroræ which are apparently at nearly equal temperatures are in all probability due to slight variations in the composition of the meteorites concerned.

A low-temperature aurora will give a spectrum generally consisting of the Mn fluting only, while at gradually increasing temperatures various other lines will be added according to the volatilities of the substances which they represent.

[*Note, April 30th.*—The existence of a fluting near wave-length 558 in the spectra of all bodies which there is evidence to show are swarms of meteorites, lends an additional interest to the appearance of a line at about that wave-length in the spectrum of the aurora. It is probable that the spectrum of the aurora has a meteoritic origin, and the 558 fluting therefore might be expected. It is seen in the aurora, in bright line stars, in new stars, in comets, and in swarms of Group II.

* 'Roy. Soc. Proc.,' vol. 45, p. 236.

This commonalty furnishes a strong argument in favour of the suggestion that the aurora line is the remnant of the manganese fluting, for it is practically certain that the fluting seen in the spectra of meteor-swarms is due to manganese.

Further, in the aurora the line which I have ascribed to the manganese fluting is associated with other lines and flutings, and it is also associated with the same lines and flutings in the spectra of meteor-swarms. These again, it is important to note, are exactly the lines and flutings which are brightest in the spectra of meteorites in the laboratory at the temperature at which the manganese fluting is best visible.

These shortly are the reasons why I suggested that further enquiry was necessary on this point, and the importance of Dr. Huggins's observation is therefore very great.]

IV. "The Accurate Determination of Carbonic Acid and Moisture in Air." By J. S. HALDANE, M.A., M.B., and M. S. PEMBREY, Pell Exhibitioner of Christ Church, Oxford. (From the Physiological Laboratory, Oxford.) Communicated by Professor J. BURDON SANDERSON, F.R.S. Received April 8, 1889.

V. "On the Spectrum, Visible and Photographic, of the Great Nebula in Orion." By WILLIAM HUGGINS, D.C.L., LL.D., F.R.S., and Mrs. HUGGINS. Received April 11, 1889.

[PLATE 1.]

I have added the name of Mrs. Huggins to the title of the paper, because she has not only assisted generally in the work, but has repeated independently the delicate observations made by eye.

In the year 1882 I had the honour to lay before the Royal Society a note on the photographic spectrum of this nebula, in which I described a new bright line in the ultra-violet, to which I gave a wave-length of about 3730. In addition to this new line, the lines of hydrogen, $H\beta$ and $H\gamma$, which I had discovered by eye in my early observations on the visible spectrum, were to be seen upon the plate.

On account of the faintness of the object the slit had been made rather wide, and for this reason the character of the line and its position, as I stated in the paper, could not be ascertained with the accuracy which I desired.

On the 5th February, 1888, a photograph of the spectrum of this

nebula was obtained with a narrow slit; the same apparatus, so far as the essential parts, which were described in my paper on the "Photographic Spectra of the Stars,"* being employed.

In this photograph, in addition to the strong line about λ 3730, a pair of less conspicuous lines is seen on the less refrangible side of the strong line.

The continuous spectra due to the two of the four bright stars of the Trapezium which fell upon the slit are present.

Across these continuous spectra at least four groups of bright lines can be seen, of which the greater number can be traced into the nebula for some little distance from the stellar spectra.

It is scarcely necessary to state the importance of this observation as showing that these stars of the Trapezium are not merely optically connected with the nebula, but are physically bound up with it, and are very probably condensed out of the gaseous matter of the nebula. This observation would seem also to show that the nebula, as a whole, may not be at a distance from us greater than that which we should attribute to such stars, if they occurred alone in the heavens.

The first group, of six lines, occurs between λ 4116 and 4167. The lines of this group do not extend far from the continuous star spectra, with the exception of two lines. These can be seen faintly in another photograph taken in 1889. Beyond there is a fainter group, probably of four lines a little beyond *h*. I am pretty sure that these lines extend into the nebula. The third group from λ 3896 to 3825, of which I have endeavoured to measure ten lines, is faint, but here there is no doubt that the same lines are present in the adjoining nebular matter. There are two lines a little more refrangible than the strong line seen in 1882, at about λ 3709 and λ 3699. I have a suspicion of a faint group about this place, and also of another group on the less refrangible side of G.

I shall discuss further on the probable chemical significance of these lines. The lines of this photograph are shown in Spectrum No. 1 of the folding Plate.

During the time that Orion was favourably situated for observation in the season of 1888 and in that of the present year, the unusual continuance of bad weather has made it impossible for me to give so complete an account of the spectrum of the nebula in the photographic region as a few really fine nights would have enabled me to do. However, on February 28th of the present year I obtained another photograph, the slit being very narrow, which gives some more new information of the nature of its spectrum. I was astonished on looking at the photograph not to see the strong line about λ 3730, which was by far the most conspicuous feature of the photograph taken in 1888. The pair of lines near it on the less refrangible side,

* 'Phil. Trans.,' 1880, p. 672.

which I found for the first time in 1888, are present; and on a further scrutiny of the plate I discovered two other pairs of lines, most probably rhythmically connected with them, in the still more refrangible region, the last pair, accompanied by a third line, being near the ultra-violet limit of ex-terrestrial light.*

I was also able to see faintly two of the bright lines which I have described as present across the continuous spectra of the brighter stars of the Trapezium in my photograph of 1888. It is not quite certain whether these very faint and short lines are really due to the matter of the nebula proper, or have come upon the plate in consequence of the stars of the Trapezium having fallen accidentally upon the slit for a time too short to impress the continuous part of their spectra. No trace of a continuous spectrum can be seen upon the plate, but these lines in the plate of 1888 do extend beyond the continuous spectra of the stars of the Trapezium.

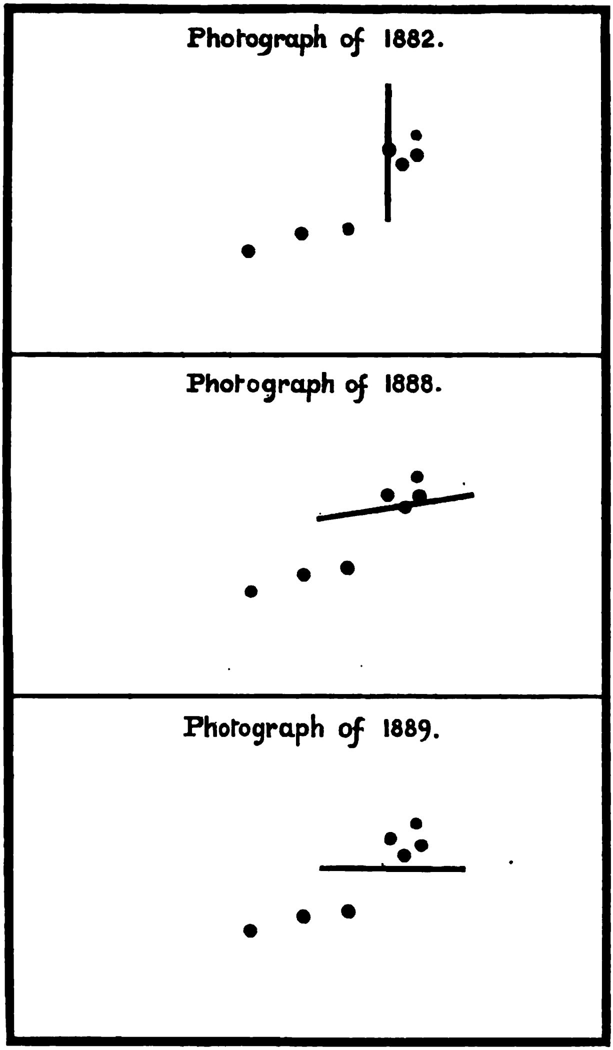
In the diagram which follows I have indicated the positions of the slit upon the nebula relatively to the Trapezium and the well-known three stars near it, for the photographs of 1882, 1888, and 1889.

I regret extremely that bad weather has made it impossible for me to work out the circumstances on which depended the disappearance of the strong line about λ 3730. Both the photographs which show this line include two stars of the Trapezium, and it may possibly be that this strong line is associated with the groups near it in the spectra of the stars, and may therefore come out in those parts of the nebula only which are more condensed. A few photographs with the slit differently placed upon the nebula would doubtless have thrown light upon this point. The suggestion presents itself strongly that the mottled and broken-up character of the nebular matter, shown in Lord Rosse's drawing from eye observations, and much more strikingly brought out in the recent photographs of Mr. Common and Mr. Roberts, may be connected with differences of spectrum in the photographic region, though in the visible region there is no known alteration of the spectrum of the four bright lines, except, it may be, some small differences of relative brilliancy of the lines.

Until next winter we cannot go beyond the new information which these photographs give to us. On the plate of the photograph of 1889 two pairs of spectra for comparison were taken:—two spectra, one above and one below the nebular spectrum, of burning magnesium; and two spectra, similarly placed, of the light of the sky.

From the photographs of 1888 taken with a narrow slit, the position which I gave in 1882 to this line is shown to be, as I expected from the wide slit then used, approximate only. I find from the later photograph that the wide slit had caused the strong

* "On the Limit of Solar and Stellar Light in the Ultra-violet Part of the Spectrum." *Infrà*, p. 133.



line to unite with a line near it, and that in 1882 I measured the middle of the broad band produced by the union of the wide images of two lines. Its position is about six tenth-metres more refrangible. It does not therefore agree, as I then suggested, with the hydrogen line ζ in my spectra of white stars. A statement of the position of this line relatively to the magnesium-flame triplet will be given further on, when I come to discuss the comparison of this spectrum with that of the nebula.

The position of the pair of lines a little less refrangible than this strong line, seen with it in the photograph of 1888, and present without the strong line in the photograph of 1889, and the positions of the two other more refrangible pairs, presumably connected with the first pair, are given in the following table :—

1st pair about	λ 3752·0 3741·0
2nd pair about	3285·0 3275·0
Line at about........	3060·0
3rd pair about	3053·0 3047·0

These three pairs of lines are shown in Spectrum No. 3 of the Plate.

In both photographs I suspect the indications of other lines, which are too faint to permit any certain conclusion to be formed about them, whether they are true lines, or imperfections only of the film.

[The continuous spectra of the stars of the Trapezium can be seen on the plate from about F to λ 3574; but they are very faint beyond λ 3660.—May 7.]

The Visible Spectrum.

a. Brightest line.—In 1872,* I stated as the result of numerous direct comparisons of this line with the brightest line in the spectrum of nitrogen that the nebular line was "sensibly coincident with the middle of the less refrangible line of the double line of nitrogen." To avoid repetition I will call this line N_1. Except where it is otherwise stated, I use this line of nitrogen simply as a fiducial point in the spectrum, without any reference to its chemical significance.

In a still more critical examination of the position of the nebular line for the purpose of determining whether there was any indication of relative motions of the gaseous nebulæ in the line of sight, I found some experimental difficulty from the circumstance that the nebular line is narrow and defined while N_1 is nebulous. I was fortunate to find a more suitable fiducial line of comparison in a narrow line of lead which falls almost upon the middle of N_1.† In December, 1872, I compared this line directly with N_1, and found it sufficiently near in position to serve as a fiducial line of comparison.

Six other gaseous nebulæ were also examined, each on several

* "On the Spectrum of the Great Nebula in Orion, &c.," 'Roy. Soc. Proc.,' vol. 20, 1872, p. 383.

† "On the Motions of some of the Nebulæ towards or from the Earth," 'Roy. Soc. Proc.,' vol. 22, 1874, p. 252.

nights, with the result that "in no instance was any change of relative position of the nebular line and the lead line detected."*

In the simultaneous observation of the nebular line and the lead line it was found if the lead line was made rather less bright than the line of the nebula, the small excess of apparent breadth of this latter line appeared to overlap the lead line to a very small amount on its less refrangible side, so that the more refrangible sides of the two lines appeared to be in a straight line across the spectrum. The closeness of position of the two lines was shown by the observation that when the line of the nebula passed across the field of the spectroscope, and the lead line was thrown in, the lead line was not seen, but only an increase in brightness of the nebular line. By comparing the end of the nebular line near the Trapezium where it is refined to a point, I estimated that the difference of position of the middle of the lead line and that of the nebular line might be possibly from λ 0000·2 to λ 0000·3.† Some recent measures of the position of the lead line with the middle of N_1 show that the lead line is about λ 0000·12 more refrangible.

These direct comparisons of the nebular line with the lead line confirmed, therefore, my former conclusion, that the brightest line in the gaseous nebulæ is very near N_1, when seen under a dispersion equal to nearly eight prisms of 60, namely, 36° 25′ from A to H.

This result is based on direct comparisons, on twenty-four different nights, with N_1 or with the line of lead.

The wave-length of N_1 has been determined by Kirchhoff, Thalén, and by myself. Watts' reduction of my measure to wave-lengths is clearly not accordant with my measures of air lines immediately preceding and following this line. I have therefore reduced my original measure to wave-lengths and find for N_1 the value λ 5004·5.

Kirchhoff	5004·6
Thalén	5005·1

Thalén's value is clearly too high, as Thalén gives for the lead line coincident nearly with N_1 λ 5004·6, and N_1 is seen on the more refrangible side of the solar iron line given by Ångström as λ 5004·9. In Ångström's map N_1 is laid down on the more refrangible side of the iron line 5004·9, at about 5004·5. The same position is given to N_1 in Kirchhoff's map.

I have made a new determination of the position of N_1, using the second spectrum of a grating 17,300 to the inch, relatively to the solar iron line at 5004·9 according to Ångström.

The value came out λ 5004·6, which agrees with Kirchhoff's value,

* "On the Motions of some of the Nebulæ towards or from the Earth," 'Roy. Soc. Proc.,' vol. 22, 1874, p. 253.

† *Ibid.*, p. 252.

and with Thalén's measure of the lead line which falls upon it, and also with the maps of Ångström and of Kirchhoff.

The wave-length of the brightest nebular line may therefore be taken at from

λ 5004·6 to λ 5004·8 .... (1).

The micrometric measures of this line, given by D'Arrest, Vogel, and Copeland, agree closely with this value.

D'Arrest's* mean value..........	5004
Vogel's "	5004
Copeland's† "	5004

b. Second line.—In 1872,‡ I stated that I had found this line, by comparison with a line of barium and subsequently with an iron line, to have

A wave-length of	λ 4957·0.... (2).
D'Arrest's mean value from micrometric measures	λ 4956·6
Copeland's " " "	4958·0

c. Third line.—In my original paper§ "On the Spectra of some of the Nebulæ," in 1864, I showed, by direct comparison with hydrogen, that this line is undoubtedly the line of that gas at F of the solar spectrum. This observation was afterwards repeated,|| and has been confirmed by the photographs of 1882 and 1888.

The wave-length of this line is therefore	λ 4860·7 .... (3).
D'Arrest's value from measures.......	4860·6
Copeland's " "	4861·0

d. Fourth line.—In 1872,¶ I stated that I had satisfied myself of the coincidence of this line with $H\gamma$, but, on account of its faintness, it is very satisfactory to find this observation of coincidence confirmed by the photographs taken in 1888 and 1889. There can be no doubt that this is a line of hydrogen, and that

The wave-length therefore is..........	4340·1 .... (4).
Copeland's mean value	4342·0

Dr. Copeland gives the measures of two still fainter lines which he has seen in this nebula, namely, one at λ 5874, possibly coincident

* 'Undersögelser over de nebulose Stjerner.' Copenhagen, 1872, p. 23.
† 'Monthly Notices, R.A.S.,' vol. 48, p. 361.
‡ 'Roy. Soc. Proc.,' vol. 20, 1872, p. 385.
§ 'Phil. Trans.,' 1864, p. 437.
|| 'Phil. Trans.,' 1868, p. 545.
¶ 'Roy. Soc. Proc.,' vol. 20, 1872, p. 385.

with D_3, and a line at λ 4476. (See also Mr. Taylor, 'Monthly Notices, R.A.S.,' vol. 49, p. 125.) I defer the consideration of these and other faint lines which I have often suspected in the faint continuous spectrum of the nebula, as in consequence of the great strain upon the eyes from my recent direct comparison of the spectrum of the nebula with the spectrum of burning magnesium, I was not able during the very few fine nights when Orion was favourably situated to undertake an examination for these very faint lines.

Comparisons with the Magnesium-flame Spectrum.

In 1882, Dr. Copeland in his paper on Schmidt's Nova Cygni,* remarked in a footnote, "that it is worthy of note that this line (λ 5006·5 of burning magnesium) almost absolutely coincides with the brightest line in the planetary nebulæ."

This line, namely, the bright edge of the first band in the magnesium-flame spectrum, is very near in position to the brightest nebular line. We have seen that the wave-length of this line in the nebulæ (1) is 5004·6 to 5004·8; now the wave-length of the end of the magnesium-flame band is 5006·5, consequently it does not coincide with the nebular line but falls on the less refrangible side at a distance of λ 0002 nearly from that line.

The wave-length of the termination of the magnesium-flame band is, as determined by—

Lecoq de Boisbaudran	5006·0
Watts	5006·5
Liveing and Dewar†	5006·4

I have recently redetermined the position of the end of the band, by direct comparison with the solar iron line given by Ångström at λ 5006·58.

My result places the magnesium-flame band line at λ 5006·5 ... (5).

In a paper read before the Royal Society in 1887,‡ Mr. Lockyer says:—"Only seven lines in all have been recorded up to the present in the spectra of nebulæ, three of which coincide with lines in the spectrum of hydrogen and three correspond to lines in magnesium. The magnesium lines represented are the ultra-violet low-temperature line at 373, the line at 470 and the remnant of the magnesium fluting at 500, the brightest part of the spectrum at the temperature of the

* 'Copernicus,' vol. 1, p. 109.

† 'Roy. Soc. Proc.,' vol. 44, 1888, p. 245.

‡ "Researches on the Spectra of Meteorites: a Report to the Solar Physics Committee; communicated to the Royal Society at the request of the Committee," 'Roy. Soc. Proc.,' vol. 43, p. 118.

bunsen burner." At page 137 (*loc. cit.*) Mr. Lockyer says :—" In the nebulæ we deal chiefly with lines seen in the spectrum of magnesium at the lowest temperature."

In a later paper in 1888* Mr. Lockyer states :—" In a paper communicated to the Royal Society on November 15th, 1887, I showed that the nebulæ are composed of sparse meteorites, the collisions of which bring about a rise of temperature sufficient to render luminous one of their chief constituents—magnesium. This conclusion was arrived at from the facts that the chief nebular lines are coincident in position with the fluting and lines visible in the bunsen burner when magnesium is introduced, and that the fluting is far brighter at that temperature than almost any other spectral line or fluting of any element whatever."

Although the number of direct comparisons which I had made of the brightest line in the nebulæ with N_1 and with the lead line, not to speak of the accordant results of the micrometric measures of other observers, left great doubt in my mind whether this line could be coincident with "the remnant of the magnesium fluting at 500," really at 5006·5, yet I thought it desirable to undertake the laborious task of comparing, with the necessary care and precautious, the nebular line directly, in the spectroscope attached to the telescope, with the spectrum of burning magnesium.

Arrangements were made by which the light from burning magnesium was thrown into the telescope from the side and then reflected down, under conditions similar with the light from the nebula, upon the slit of the spectroscope. By this arrangement any flexure in the tube connecting the spectroscope with the telescope would affect both spectra alike. The coincidence in position of the spectrum from burning magnesium with that of a heavenly body to which the telescope was so directed that its light fell upon the slit of the spectroscope, was tested with great care on several occasions by comparing the three bright lines of magnesium with the corresponding lines, b_1, b_2, b_4, in the spectrum of the moon. Indeed, to prevent any possible error in the observation of apparent want of coincidence of the nebular line, if the light from the burning magnesium should by an accident so come upon the slit as to bring its spectrum in a very minute degree on the less refrangible side of its true position relatively to the nebular line to be observed with it, the arrangement was purposely made that the lines of magnesium were seen to fall upon the corresponding dark lines at b in the moon, a very little on the more refrangible side of the middle of those lines. This state of things would diminish a little the interval which should be seen between the nebular line and the edge of the

* "Suggestions on the Classification of the various Species of Heavenly Bodies." 'Roy. Soc. Proc.,' vol. 44, p. 21.

magnesium-flame band, and so make the determination more difficult; but if under such circumstances, the nebular line was seen on the more refrangible side of that of magnesium the observation would be much more trustworthy, for in the case of coincidence with magnesium the line would have appeared towards the opposite and less refrangible side of the magnesium line, broadening the magnesium line on this side. I considered that the comparison could be made most satisfactorily by the complete superposition of the two spectra, that from burning magnesium being gradually reduced in brightness by the interposition of coloured glass screens, until the ground of the spectrum between the successive bright lines of the band of the magnesium-flame spectrum was sufficiently subdued to allow of the nebular line being seen upon it.

Under these circumstances, if the nebular line had the position which my direct comparisons and the micrometric observations of other observers assign to it, it would be seen as a bright line at a very small interval within the line ending the band, and to the observer the band would appear to commence with a double line.

This direct comparison was first successfully made on March 6, 1889. The observations were made with the 15" refractor belonging to the Royal Society. The spectroscope used has two compound Grubb prisms, each with 5 square inches of base, and giving nearly twice the dispersion of a single prism of 60°, namely, 9° 20′ from A to H; and collimator and telescope of 1·25-inch aperture. An eyepiece magnifying eighteen times was employed. The nebular line was brought upon the cross-wires, and when carefully focussed and clearly seen, the light from burning magnesium was thrown in. This observation is one of great difficulty, especially as the interval to be observed had been purposely reduced by causing the magnesium to fall, for the sake of the greater trustworthiness of the observations, on the more refrangible side of its true position. Although I consider the results to be satisfactory, I prefer to say that I, and Mrs. Huggins independently, believed fully at the time that we saw the appearance which all former observations of this line led me to expect, namely, the nebular line to fall within the termination of the magnesium band, and to form with the band-boundary a double line. The relative positions of the two spectra are represented in the diagram across the page. The line at the end of the magnesium band was then brought upon the cross-wires, without any attention being given to the nebular line; when the burning magnesium went out, the nebular line was seen to be at a measurable distance to the left of the intersection of the wires, namely, on the more refrangible side.

When the object-glass of the telescope was covered, the magnesium band presented its usual appearance, namely, terminating in a single


5010
5000
4990
4980
4970
4960
4950
4940
4930
Nebula Line
Nebula Line

line. These comparisons were repeated and confirmed generally on March 9, March 11, and March 16. On March 9, a single successful comparison was made with a more powerful spectroscope, giving a dispersion equal to nearly eight prisms of 60°. [Comparisons have been made since with the planetary nebula in Hydra. The short line of the nebula was found to fall within the termination of the magnesium band at about the small distance which corresponds to the known position of the two lines.—April 26.] On all these nights the comparisons were repeated independently and fully confirmed by Mrs. Huggins.

These comparisons can be successfully imitated in the laboratory by directing a spectroscope of sufficient power to the line of lead which the nebular line is sufficiently near, the slit being narrow and the electrodes of lead near each other; and then causing, with the necessary precautions, the light of burning magnesium to fall also upon the slit. The lead line will be seen to fall within the end of the band, and to form with it a double line.

It may be mentioned in this place that this line of lead, and the iron line at 4957 at the position of the second nebular line, can be conveniently used in the laboratory in any chemical research on the nature of the nebulæ. No terrestrial line which does not fall almost exactly at these positions in the spectrum can have any claim to further consideration.

It might be suggested that the want of coincidence observed between the nebular line and the magnesium band, amounting to λ 0001·9 nearly, might be due to a motion of translation of the nebula towards the earth. The motion required to produce this shift of position is about sixty-seven miles in a second.

[The earth's motion at the time of comparison with the magnesium-flame band may be taken at about 17 miles in a second of recession from the nebula. This motion would bring the nebular line nearer the red, and diminish the apparent interval between that line and the termination of the band. If the nebula has a motion of approach, the earth's recession would bring the line back again, to an extent corresponding to about 17 miles in a second, towards its true place.—May 13.]

I showed in my paper on this subject in 1874,* that in the case of the Orion nebula and six other gaseous nebulæ, namely, 4234, 4373, 4390, 4447, 4510, 4964, of Sir J. Herschel's 'General Catalogue of Nebulæ,' "in no instance was any change of relative position of the nebular line and the lead line detected." We should have to resort, therefore, to the overwhelmingly improbable supposition that all

* 'Roy. Soc. Proc.,' vol. 22, p. 253.

seven nebulæ were approaching the earth with velocities such that, having respect to the earth's motion at the different times of observation, they all gave a sensible shift corresponding to 67 ± 15 miles in a second. There is little doubt in my mind, therefore, from these comparisons, which, considering the strong evidence we possessed before of the relative positions of the nebular line and of the magnesium line, are strictly speaking supplementary and confirmatory evidence only, that this line of the gaseous nebulæ is not produced by "the remnant of the magnesium fluting."*

In the diagram on page 134 ('Roy. Soc. Proc.,' vol. 43), Mr. Lockyer represents this nebular line followed by fine lines, which gives it the appearance of a fluting similar to that of the magnesium band placed above. I am unable to find in the paper any authority for this representation of the line. In another place† Mr. Lockyer says: "On one occasion, at Greenwich, it was recorded as a fluting in the spectrum of the nebula in Orion." Mr. Maunder's words are:‡ "None of the

* [The following observations were made at Greenwich in 1884 on the motion of the nebula in the line of sight:—

"February 15. Mean of four observations, 31 miles of approach."

Remarks: Measures purely tentative.

"February 18. Mean of four observations, 51 miles of approach."

Remarks: The F line in the spectrum of the nebula was faint, very much fainter than the line at λ 5005. The measures, therefore, are not trustworthy.

"March 10. With neither one nor two prism trains, after very careful direct comparison (the light from the comparison tube being weakened until it could be compared directly with the light from the nebula), could any displacement be detected; the coincidence of two spectra was evidently very close.

"March 12. Spectrum too faint for measures. The hydrogen spectrum was brought down to almost exactly the same intensity as the light from the nebula, and direct comparison showed coincidence as complete as could be detected considering the faintness of the two spectra. . . No part of the nebula which was sufficiently bright to show the pointer well on the 5005 line showed any *marked* displacement, but at a point a little preceding the Trapezium it was thought that the pointer did not seem perfectly central on the line but a little (perhaps $\frac{1}{10}$, certainly not more) towards the red" ('Greenwich Spectroscopic and Photographic Results,' 1884).

Also in 1887, six determinations on the same night.

"Oct. 25.	Measured	37·8	miles of	approach.	Estimated	24	miles of	approach.
„	„	8·3	„	recession.	„	9·3	„	recession.
„	„	7·1	„	„	„	9·3	„	„
„	„	38·4	„	approach.	„	24·0	„	approach.
„	„	24·5	„	„	„	24·0	„	„
„	„	10·8	„	recession.	„	9·3	„	recession.

Remarks: Lines in nebula very faint and bisections very rough."

('Greenwich Spectroscopic and Photographic Results,' 1887).—May 13.]

† Programme Royal Society Soirée, May 9, 1888, p. 12.

‡ 'Greenwich Spectroscopic Results,' 1884, p. 5.

lines" (with two-prism train) "are very sharp. λ 5005 showed a faint fringe mainly on the side nearer the blue."

Mr. Maunder has recently sent a note to the Royal Astronomical Society, in which he explains that the observation was made with a second half-prism added to the half-prism spectroscope. He says: "The three principal lines of the nebular spectrum were seen as very narrow bright lines, but none of them were perfectly sharp, each showed a slight raggedness at both edges; but in the case of the line near λ 5005 it was clear that this fringe, or raggedness, was more developed towards the blue than towards the red. In the case of the other two lines, they were not bright enough for it to be possible to ascertain whether the fringes were symmetrical or not. But λ 5005 was clearly a single line. There was no trace of any bright line, or series of bright lines, close to it on either side; no trace of a fluting, properly so called. The entire line, fringes and all, was only a fraction of a tenth metre in total breadth."*

[It should be noticed that the instrumental conditions under which Mr. Maunder observed showed the second and third line "not perfectly sharp, but with a slight raggedness at both edges."—May 13.]

My own observations of this line, since my discovery of it in 1864, with different spectroscopes up to a dispersion equal to eight prisms of 60°, show the line to become narrow as the slit is made narrow, and to be sharply and perfectly defined at both edges.

As some importance attaches to the precise character of this line, I wrote to Professor H. C. Vogel for permission to quote the result of his experience, which has been nearly as long as my own, of the character of this line. He says in his reply, dated 20th March, 1889: "Beeile ich mich Ihnen mitzutheilen, dass meine langjährigen Beobachtungen über die Spectra der Gas-Nebel *vollkommen* mit den Ihrigen darin übereinstimmen, dass die Nebellinie λ 5004 schmal, scharf und nicht verwaschen ist. Auch D'Arrest hat in seiner Untersuchung über die Nebel-Spectra (Kopenhagen, 1872) nicht erwähnt dass die hellste Nebellinie unscharf sei."

Dr. Copeland permits me to quote the following sentences of a letter dated March 19, 1889:—"Respecting the appearance of the line λ 5004 in the spectrum of the Orion nebula, I may say that I have always drawn and seen it quite sharp and well defined on both edges. About nine years ago I made a special effort to divide it, if possible, with a large spectroscope in which the viewing telescope was 3 inches in aperture. The lines were then seen as sketched." (The diagram shows the nebular lines with sharply ruled lines for edges.) "They were drawn by holding the note-book 10 inches from

* 'Monthly Notices R.A.S.,' vol. 49, 1889, p. 308.

the left eye, in such a position that the image seen in the instrument with the right eye was apparently projected on the paper. If I had noticed any peculiarity about λ 5004, it would certainly have been noted."*

In an early observation of the dumb-bell nebula Professor Vogel, indeed ('Beobachtungen zu Bothkamp,' p. 59, 1872), describes this line as less defined towards the violet side. In a letter (April 3, 1889) Professor Vogel says this appearance of the line was probably due to a slit not sufficiently narrow. He says that he re-examined this line in his observations with the great Vienna refractor, and that it did not then appear otherwise than defined and narrow.

The other line in the spectrum of the nebulæ upon which Mr. Lockyer mainly relies for the presence of magnesium is the line shown in my photographic spectrum of 1882,† and to which I assigned the wave-length of about 3730. Mr. Lockyer says of this line:‡ "In the Bunsen as ordinarily employed the fluting at 500 far eclipses the other parts of the spectrum in brilliancy, and at this temperature, as already observed by Messrs. Liveing and Dewar, the ultra-violet line visible is that at 373." Passing by a minor point, which Liveing and Dewar have already pointed out,§ namely, that their observation was made at the higher temperature of burning magnesium, this statement is insufficiently complete, for what occurs at this part of the spectrum, and is characteristic of the magnesium-flame spectrum, is a triplet, of which the line given by Liveing and Dewar at about 3730 is the least refrangible member.

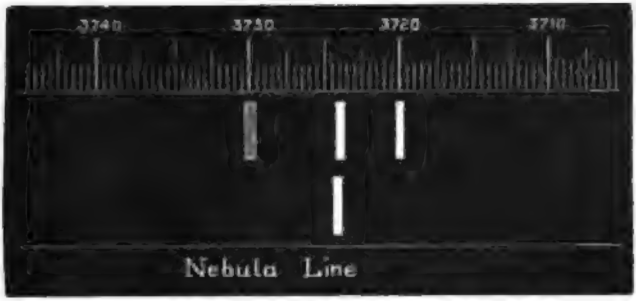
In the accompanying Diagram I give a representation of this triplet at the wave-lengths given by Liveing and Dewar, namely λ 3730, 3724 and 3720. In the photograph of 1888, in which the strong line can be seen distinct from the lines near it, the line is found to be very near the middle line of the triplet. I have therefore assigned to this line the position of about λ 3724. This line appears pretty strong, and therefore if it were really one of the lines of the triplet, the other two members of the triplet should have appeared on the plate. On one side of the star-spectra this line is a little broader than on the other

* Mr. Taylor, late of the South Kensington Laboratories, observing at Sir Henry Thompson's observatory in November, 1888, says: "The 5001 line is by far the brightest in the spectrum. It is never seen sharp, but with the narrowest slit always has a fluffy appearance, this being much more marked on the blue than on the red edge. This line was most carefully examined for evidence of structure, but was always found to be single, and no decided evidence of fluting structure could be made out. It may be that greater dispersion may show structure, but with the dispersion used here no structure could be seen." 'Monthly Notices R.A.S.,' vol. 49, p. 125.

† 'Roy. Soc. Proc.,' vol. 33, p. 425.

‡ 'Roy. Soc. Proc.,' vol. 43, 1887, p. 122.

§ 'Roy. Soc. Proc.,' vol. 44, 1888, p. 244.



side, but as a similar appearance is presented by G, and the stronger of the lines of the group, it may arise from some optical or photographic cause. The line at 3724 impresses me strongly as a single line, and there is certainly no trace of the first line of the triplet at 3730. The line appears to me stronger where it is upon the star-spectra.

As therefore there seems to be little doubt that the "remnant of the fluting at 500" is not coincident with the brightest nebular line, and the next most characteristic group of this spectrum, the triplet at 3720, 3724, and 3730, according to Liveing and Dewar, does not appear to be present in the photographs, we may conclude that the remarkable spectrum of the gaseous nebulæ has not been produced by burning magnesium.*

I should mention that Mr. Lockyer attributes one other line occasionally seen in the gaseous nebulæ to the flame spectrum of magnesium, namely, a very faint line at about λ 4700. Now, according to my experience, it is only in the spark and arc that a line of magnesium appears at this place, a condition of the spectrum when the lines at *b* are very conspicuous, and the band at λ 5006·5 is usually absent. When, however, the spark is taken in magnesium chloride, the band is present under some conditions, but the triplet at *b* is always bright. I therefore consulted Professor Liveing, who

* On the narrower basis of the magnesium spectrum only, Professors Liveing and Dewar point out that: "the appearance of a line in the position of the first band without any trace of the second band, which is nearly as bright as the first, and without any trace of the *b* group, is quite sufficient to create a suspicion of mistaken identity when Mr. Lockyer ascribes the sharp green line in the spectrum of nebulæ to this band of magnesia. This suspicion will be strengthened when it is noticed that the line in question is usually in nebulæ associated with the F line of hydrogen, if it be borne in mind that the spark of magnesium in hydrogen does not give the bands, and that the oxyhydrogen flame hardly produces them from magnesia when the hydrogen is in excess." ('Roy. Soc. Proc.,' vol. 44, p. 245.) Mr. Taylor records a brightening of the continuous spectrum of the nebula at λ 5200, which he suggests may be magnesium. But this position is twenty-five units from that of the middle of the magnesium triplet at "*b*." ('Monthly Notices R.A.S.,' vol. 49, p. 125.)

says: "I have never seen the line at λ 4703 in the spectrum of the magnesium flame. As it is a conspicuous line in the arc and spark, we looked for it in the flame, but did not find it."

With reference to the second nebular line at λ 4957, Mr. Lockyer says:* "The lines at 500 and 495 have been seen in the glow of the Dhurmsala meteorite when heated, but the origin of 495 has not yet been determined." And further (at p. 135): "I should add that the line at 495 makes its appearance much more rarely than the one at 500 in meteorite glows." In the diagram on the same page this line is represented as coincident with the nebular line.

The circumstance of a line appearing at 495 can scarcely be regarded, considering the very great number of spectral lines, as amounting to a presumption that the material to which it is due in the meteorite is the same as that present in the nebulæ which gives the line at 4957. If it should be shown that the unknown substance in the meteorite gives rise to a line at the position of the nebular line, namely, λ 4957, in that case the observation would have sufficient importance to make it desirable to compare the spectrum of the meteorite directly with that of the nebula.

Lines Observed and Photographed in the Spectrum of the Nebula.

Line measured by Dr. Copeland, probably D_3			λ 5874·0
Brightest line			5004·6 to 5004·8
Second line			4957·0
Third line, Hβ			4860·7
Fourth line, Hγ			4340·1
Line measured by Dr. Copeland			4476·0
Strong line in photographs 1882 and 1888 about			3724·0
Line in photographs 1888 about		„	3709·0
„ „		„ „	3699·0
Photograph 1889.	1st pair..	about	3752·0
		„	3741·0
„	2nd pair..	„	3285·0
		„	3275·0
	Line at..	„	3060·0
„	3rd pair..	„	3053·0
		„	3047·0
Photograph 1888........... Lines across star spectra, 1st group		„	4116·0
		„	4123·0
		„	4130·0
		„	4142·0
		„	4154·0
		„	4167·0

* 'Roy. Soc. Proc.,' vol. 43, p. 133.

2nd group	approximate	λ 3998·0
	"	3988·0
	"	3975·0
	"	3959·0
3rd group	"	3896·0
	"	3887·0
	"	3878·0
	"	3870·0
	"	3859·0
	"	3854·0
	"	3848·0
	"	3842·0
	"	3832·0
	"	3825·0

Chemical Significance of the Lines.

Until I can obtain more photographs taken on different parts of the nebula, I wish to be understood to speak on this point with much hesitation, and provisionally only. We know certainly that two of the lines are produced by hydrogen. The fineness of these lines points to a high temperature and condition of great tenuity of the hydrogen from which the light was emitted. This condition of the hydrogen may give us a clue as to the probable interpretation of the other lines. These may come from substances of very low vapour-density, and under molecular conditions which are consistent with a high temperature. It is in accordance with this view that the recent measures of Dr. Copeland, since confirmed by Mr. Taylor (*loc. cit.*), show with great probability that the line known as D_3, which has been supposed to indicate some substance of low vapour-density, which shows itself only at the hottest region of the sun, is present in the nebular spectrum. The great simplicity of the three pairs of lines seen in the photograph of 1889 suggests a substance of a similar chemical nature.

If hydrogen can exist at half its usual vapour-density, with a molecule of one atom only, we might possibly expect to find it in some of these bodies, but at present we do not know what its spectrum would be in such a condition. It may be possibly that it is in molecular states of our elements other than those we are acquainted with that we may have to look for an interpretation of some of the lines of these bodies.

[With respect to the groups of lines which cross the star spectra, any statements must also be provisional only.

These lines are distinct and fairly strong in the star spectra, and extend, some farther than others, into the adjoining nebular

matter. Whether they are peculiar to these particular stars and the matter close about them, or whether they will be found everywhere in the nebula, or in certain parts of greater condensation only, can be known only from future photographs.

The first group shows some general agreements with a strong iron group, but there are also formidable discrepancies.

The position of the third group suggested the well known cyanogen group, especially as this group, beginning at λ 3883, is the first to appear under the chemical conditions which might have been conceived to exist under circumstances of condensation.* Under these conditions this group appears alone in a photograph, without the less refrangible group, as was probably the case in the photograph I took of Comet II, 1881. I therefore took a photograph of an oxy-coal-gas flame, the coal-gas having passed through ammonia, and a magnesium-flame spectrum on the same plate for comparison.

On comparing this photograph with that of the nebula it was seen by eye, and afterwards confirmed by measurement, that the nebula group begins sooner by one strong line than the cyanogen group, and presents besides in the relative strength and grouping of the lines a distinctly different character. The evidence appears to me to be against attributing these lines to cyanogen.

I took great pains to ascertain if the group of lines which accompanies the triplet of the magnesium-flame spectrum could be made to agree with the much longer group of lines in the nebula at this part of the spectrum. Again, as in the case of the cyanogen group, the whole aspect of the grouping of lines is quite different. The groups begin and end differently, and the relative strength of different parts of the group is not the same. The great increase of strength which is seen in the middle of the magnesium group is not present at the corresponding part of the nebula group. I do not think therefore there should be much weight given to the near positions of several individual lines of the two groups, which in the case of so close a grouping might well be accidental, especially as the wave-lengths can be but approximate only.

[The strongest lines of the magnesium-flame group are those forming the triplet which appears also in the spark and the arc spectrum. A nebular line is near the middle line of the triplet, but there are no lines corresponding to the other two lines of the triplet. The other lines of the flame group are too faint to be expected to appear, unless the triplet at 3720—3730 were strong upon the plate. —May 13.]

The three pairs of lines in the photograph of 1889, which are

* See Liveing and Dewar, 'Roy. Soc. Proc.,' vol. 34, 1883, p. 128.

doubtless rhythmically connected, appear to me to possess great interest, especially if it should come to be found from future photographs that these groups are characteristic of the most tenuous part of the nebula. At present, I am not able to make any suggestion as to their chemical origin, but the suggestion presents itself that we may have to do with some molecule of low vapour-density.

The pair of lines on the more refrangible side of the line at λ 3724, may possibly be connected with the state of the nebula as it exists in the neighbourhood of the stars.—April 26.]

General Conclusions.

It seems to me premature until we can learn more of the significance of the new groups of lines, and especially of their connexion with the nebular matter generally, or with certain condensed parts only, to express more than provisional suggestions as to the nature of these nebulæ. It may be that they represent an early stage in the evolutionary changes of the heavenly bodies.

As some physical importance, in the relation of these nebulæ to each other, has been given to my inability, in consequence of insufficient optical means in my original observations in 1864, to see all three of the bright lines in some faint nebulæ, I may mention that in the case of one object, the Ring Nebula in Lyra, in which at that time the light appeared monochromatic, as only the brightest line could be certainly seen, as soon as larger means were placed at my disposal by the loan of the Royal Society telescope in 1870, I had no difficulty in seeing all three lines on any night of sufficient clearness. There is little doubt that the same cause prevented me from seeing more than the brightest line in Nebula 4572 of Herschel's 'General Catalogue.' Vogel saw two lines.*

These bodies may stand at or near the beginning of the evolutionary cycle, so far as we can know it. They consist probably of gas at a high temperature and very tenuous, where chemical dissociation exists, and the constituents of the mass, doubtless, are arranged in the order of vapour-density. As to the conditions which may have been anterior to this state of things, the spectroscope is silent. We are free, so far as the spectroscope can inform us, to adopt the hypothesis which other considerations may make most probable. On Dr. Croll's† form of the impact theory of stellar evolution, which begins by assuming the existence of stellar masses in motion, and considers all subsequent evolutional stages to follow from the energy of this motion converted into heat by the collision of two such bodies, these nebulæ would represent the second stage in which these existing

* 'Beobachtungen zu Bothkamp,' 1872, p. 59.

† 'Stellar Evolution,' 1889.

solid bodies had been converted into a gas of a very high tempera' They would take the same place, if we assume with Sir Wil Thomson* the coming together of two or more cool solid masses b velocity due to their mutual gravitation alone.

I pointed out in 1864† that the gaseous nature of these b would afford an explanation of the appearance of flat disks wit condensation which many of them present. The light emitted b portions of the gas further from us would be in part or w absorbed by the gas through which it would have to pass, in this giving to us the appearance of a luminous surface only.

In some of these bodies there is also a very faint contin spectrum, which if we had more light might be found to consi great part at least, of closely adjacent bright lines. Such is prob the nature, in part, of the apparently continuous spectrum of nebula with which this paper deals chiefly, the Great Nebul Orion.

In other gaseous nebulæ strong condensations are seen, an stronger "continuous" spectrum. When we come to nebul which the nebula in Andromeda may be taken as representative, strong bright line spectrum is absent, and we have what for c venience I called in my original observations of these bodie "continuous" spectrum, though I was careful to point out that it probably "crossed by bright or dark lines."

Out of about sixty nebulæ and close clusters observed by up to 1866, I found a proportion of about one-third, namely, ninete to present the spectrum of bright lines.‡

The stage of evolution which the nebula in Andromeda represents no longer a matter of hypothesis. The splendid photograph recen taken by Mr. Roberts§ of this nebula shows a planetary system at somewhat advanced stage of evolution; already several planets hav been thrown off, and the central gaseous mass has condensed to moderate size as compared with the dimensions it must have possesse before any planets had been formed.

[Mr. Maunder permits me to add that he does not consider the measures and estimations of the motions of the nebula taken in 1884 and 1887 of any weight, but he attaches great importance to the direct comparisons of March, 1884, which show that the nebula has but very little, if any, sensible motion in the line of sight.—May 16.]

* 'Roy. Instit. Proc.,' vol. 12, pp. 15, 16.

† 'Phil. Trans.,' 1864, p. 442.

‡ 'Phil. Trans.,' 1866, p. 383.

§ 'Monthly Notices R.A.S.,' vol. 65, p. 49.

[The diagrams have been made with care, but the positions of the lines must be taken from the tables of wave-lengths.—May 13.]

Presents, May 2, 1889.

Transactions.

Baltimore :—Johns Hopkins University. Circulars. Nos. 70–71. 4to. *Baltimore* 1889. The University.

Brussels :—Musée Royal d'Histoire Naturelle de Belgique. Annales. Tome XIV. Folio. *Bruxelles* 1887. The Museum.

Calcutta :—Indian Museum. A Catalogue of the Moths of India. Parts 4–5. 8vo. *Calcutta* 1888–89. The Museum.

Christiania :—Kongel. Norske Frederiks Universitet. Aarsberetning. 1878. 8vo. *Christiania* 1879. The University.

Cracow :—Académie des Sciences. Bulletin International. Comptes Rendus des Séances de l'Année 1889. 8vo. *Cracovie.* The Academy.

Leeds :—Geological Association. Transactions. 1888. Part 4. 8vo. *Leeds* 1889. The Association.

London :—British Museum (Nat. Hist.). Catalogue of the Marsupialia and Monotremata. 8vo. *London* 1888; Catalogue of the Chelonians, Rhynchocephalians, and Crocodiles. New Edition. 8vo. *London* 1889; Catalogue of the Fossil Fishes. Part 1. 8vo. *London* 1889; Catalogue of the Fossil Cephalopoda. Part 1. 8vo. *London* 1888. The Trustees.

Photographic Society of Great Britain. Journal and Transactions. Vol. XIII. No. 6. 8vo. *London* 1889. The Society.

Manchester :—Geological Society. Transactions. Vol. XX. Part 4. 8vo. *Manchester* 1889. The Society.

Mexico :—Sociedad Científica "Antonio Alzate." Memorias. Tomo II. Núm. 6. 8vo. *México* 1888. The Society.

Naples :—Accademia delle Scienze Fisiche e Matematiche. Atti. Vols. II, VII, VIII. 4to. *Napoli* 1865, 1878–79; Rendiconto. Anno XV–XVIII. 4to. *Napoli* 1876–79. The Academy.

Turin :—R. Accademia delle Scienze. Atti. Vol. XXIV. Disp. 6–7. 8vo. *Torino* 1888–89. The Academy.

Utrecht :—Provinciaal Utrechtsch Genootschap van Kunsten en Wetenschappen. Aanteekeningen. 1887–88. Verslag. 1887–88. 8vo. *Utrecht.* With sundry non-scientific publications. The Society.

Observations and Reports.

Calcutta :—Meteorological Office. Weather Charts of the Arabian Sea, and the adjacent portions of the North Indian Ocean. Obl. folio. *Calcutta* 1888. The Office.

Observations, &c. (*continued*).

Columbus :—Ohio Meteorological Bureau. Report for February, 1889. 8vo. *Columbus*. The Bureau.

Dun Echt :—Observatory. Circulars. Nos. 169–170. [Sheet.] *Dun Echt* 1889. The Earl of Crawford, F.R.S.

Krakatoa Eruption. A series of Pamphlets upon phenomena connected with the eruption of Krakatoa in 1883. 8vo. &c. The Krakatoa Committee.

London :—Local Government Board. Medical Department. Annual Report. 1887–88. 8vo. *London* 1888; Report on an Epidemic of Small-pox at Sheffield, during 1887–88. Folio. *London* 1889. The Department.

Meteorological Office. Hourly Readings. 1885. Part 3. 4to. *London* 1888; Weekly Weather Report. 1888. Nos. 19–38. 4to. *London*; Quarterly Weather Report. 1879. Part 3. 4to. *London* 1888; Contributions to our knowledge of the Meteorology of the Arctic Regions. Part 5. 4to. *London* 1888; Atlantic Weather Charts. Part 4. Atlas folio. [*London* 1888]; Charts showing the Mean Barometric Pressure over the Atlantic, Indian, and Pacific Oceans. Atlas folio. [*London* 1888.] With various duplicate publications to supply Deficiencies. The Office.

Milan :—R. Osservatorio Astronomico di Brera. Osservazioni Meteorologiche, 1888. 8vo. [*Milano.*] The Observatory.

Paris :—Observatoire. Rapport Annuel. 1888. 4to. *Paris* 1889. The Observatory.

Service Hydrographique de la Marine. Set of Charts. 1887–88. [Sheets.] *Paris*. The Service.

Portugal :—Commission des Travaux Géologiques du Portugal. Recueil d'Études Paléontologiques sur la Faune Crétacique du Portugal. Vol. II. Fasc. 2. 4to. *Lisbonne* 1888; Description de la Faune Jurassique du Portugal.—Mollusques Lamellibranches. Livr. 2. 4to. *Lisbonne* 1888; Quelques Notes sur la Géologie d'Angola coordonnées et annotées par Paul Choffat. 8vo. [*Lisbonne*] 1888. The Commission.

St. Petersburg :—Physikalisches Central-Observatorium. Annalen. 1887. Theil 2. 4to. *St. Petersburg* 1888. The Observatory.

Washington :—U. S. Coast and Geodetic Survey. Bulletin. No. 8. [Sheet.] *Washington* 1889. The Survey.

U. S. Geological Survey. Atlas to Monograph, No. 12. [Sheets.] *Washington* 1883. The Survey.

U. S. Patent Office. Official Gazette. 1889. Nos. 6–13. 8vo. *Washington*. Alphabetical Lists of Patentees and Inventions, for the Quarter ending September 30, 1888. 8vo. *Washington* 1889. The Office.

Observations, &c. (*continued*).

Winnipeg:—Board of Trade. Tenth Annual Report. 8vo. *Winnipeg* 1889. The Board.

Berthelot (M.), For. Mem. R.S. Collection des Anciens Alchimistes Grecs. Livr. 4. 4to. *Paris* 1888. The Author.

Blytt (A.) The Probable Cause of the Displacement of Beach-lines. Second additional note. 8vo. [*Christiania*] 1889. The Author.

Caruana (A. A.) Monograph of a Stalagmitic Cavern. Folio. [*Malta*] 1888. The Author.

Caruel (T.) Flora Italiana. Vol. VIII. Parte 2. 8vo. *Firenze* 1889. Prof. Caruel.

Dickerson (E. N.) Joseph Henry and the Magnetic Telegraph. An Address delivered at Princeton College. 8vo. *New York* 1885. The Author.

Egeson (C.) Egeson's Weather System of Sun-spot Causality. 8vo. *Sydney* 1889. The Author.

Forel (F.-A.) Les Variations Périodiques des Glaciers des Alpes. 12mo. *Berne* 1888. With Thirteen additional pamphlets. 8vo. and 4to. The Author.

Hann (J.) Untersuchungen über die Tägliche Oscillation des Barometers. 4to. *Wien* 1889. The Author.

Hirn (G.-A.) Constitution de l'Espace Céleste. 4to. *Colmar* 1889. The Author.

Hyland (J. S.) Ueber die Gesteine des Kilimandscharo und dessen Umgebung. 8vo. *Wien* [1888]. The Author.

Kops (Jan) Flora Batava. Afl. 281–284. 4to. *Leiden* [1888–89]. The Netherlands Legation.

Lewis (T. H.) Minor Antiquarian Articles, 1880–84. 8vo. [*St. Paul, Minn.*, 1889.] The Author.

McCoy (F.), F.R.S. Prodromus of the Zoology of Victoria. Decade 17. 8vo. *Melbourne* 1888. The Victorian Government.

Mensbrugghe (G. van der) Contribution à la Théorie du Siphon. 8vo. *Bruxelles* 1889. With two other Excerpts in 8vo. The Author.

Roscoe (Sir H. E.), F.R.S., and C. Schorlemmer, F.R.S. A Treatise on Chemistry. Vol. III. Part 5. 8vo. *London* 1889. The Authors.

Schübeler (F. C.) Norges Væxtrige, et Bidrag till Nord-Europas Natur- og Culturhistorie. Bind II. Hefte 2. 4to. *Christiania* 1888. Dr. Schübeler.

Schulz (J. F. H.) Zur Sonnenphysik. Part 2. 4to. *Kiel* 1888. The Author.

Soret (J. L.) Illusions que produisent le Dessin et la Peinture. 8vo. [*Genève*] 1888. With Five other Excerpts in 8vo.
The Author.

Sturm (R.) Ueber die Zahl und Lage der singulären Punkte bei den Strahlencongruenzen zweiter Ordnung. 8vo. [*Göttingen*] 1888.
The Author.

Tesař (F.) Analysis Gravitatis Terrestris. 4to. *Pragæ* 1888.
The Author.

Waterhouse (Lieut.-Col. J.) Annual Address and Review to the Asiatic Society of Bengal. 8vo. *Calcutta* 1889.
The Author.

Weise (W.) Yield Tables for the Scotch Pine. 8vo. *London* [1888].
Dr. W. Schlich.

Whitehead (J. T.) A Historical Sketch of the Congregation now meeting in the New Gravel-pit Church, Hackney, and of its successive Ministers. 8vo. *London* 1889. [Contains Biographical matter relating to Dr. Richard Price, F.R.S., and Dr. Priestley, F.R.S.]
The Author.

Woodward (H.), F.R.S., and T. R. Jones, F.R.S. Some Devonian and Silurian Fossils of North America. 8vo. [*London*] 1888.
The Authors.

Autograph Letter of Rev. John Swinton, F.R.S. (1768), and copy of a Letter from Leibnitz to Oldenburg (1676).
Mr. J. D. Enys.

Etched Portrait of Dr. T. Graham Balfour, F.R.S.
Dr. T. Graham Balfour.

May 9, 1889.

Professor G. G. STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The Right Hon. Baron Henry de Worms was admitted into the Society.

The following Papers were read :—

I. "On the Magnetic Rotation of the Plane of Polarisation of Light in doubly refracting Bodies." By A. W. WARD. Communicated by Professor J. J. THOMSON, F.R.S. Received April 13, 1889.

In repeating Villari's* experiment on the rotation of the plane of polarisation of light in a spinning disk of heavy glass, placed with its axis of rotation perpendicular to the lines of force in a magnetic field, it was observed that the incident plane polarised light became elliptically polarised. The elliptic polarisation was due to the centrifugal force which had the effect of stretching the glass along the radii of the disk and compressing it parallel to the axis of rotation. The strained glass in the magnetic field has, therefore, the double property of elliptically polarising plane polarised light, and at the same time rotating the plane of polarisation. The strained glass therefore acted like a crystal placed in a magnetic field, and so before Villari's experiment could be properly interpreted, it was necessary to examine how the elliptic polarisation and magnetic rotation affect each other. The following investigation is an attempt to solve this question, and its conclusions show that the apparent magnetic rotation in a doubly refractive medium is a periodic function of the length of the path of light in the medium. This result entirely accounts for the effects observed by Villari, and those observed by Lüdtge in a piece of compressed glass.

Let the axes of the doubly refracting diamagnetic medium be taken as those of x and y, and let the axis of z be the direction in which the light travels.

Let α be the inclination of the plane of vibration of the incident light to that of xz. The equation of the incident light may be written

* Villari, 'Rendiconti del Istituto Lombardo,' 9 June, 1870.

$$\left.\begin{aligned} x &= c\cos\alpha\cos(2\pi/\lambda)(vt-z) \\ y &= c\sin\alpha\cos(2\pi/\lambda)(vt-z) \end{aligned}\right\},$$

where c^2 is the intensity of the light, and the other symbols have their usual meanings.

Let β be the angular retardation in passing through the crystal. Then the equation of the emergent elliptically polarised light is

$$\left.\begin{aligned} x &= c\cos\alpha\cos(2\pi/\lambda)(vt-z) \\ y &= c\sin\alpha\cos(2\pi/\lambda)(vt-z+\lambda\beta/2\pi) \end{aligned}\right\}.$$

The inclination ω of the axis of this ellipse to the axis of x is given by

$$\tan 2\omega = \tan 2\alpha\cos\beta.$$

In this equation β is a function of λ and z, viz., $(2\pi z/\lambda)(\mu_1-\mu_2)$ where μ_1 and μ_2 are the refractive indices along the axes of x and y respectively. We may, therefore, put β equal to kz where k is a constant for the same medium and wave-length. Hence ω is a function of z, and we can find the increase in ω due to an increase dz in z. It is given by

$$2d\omega = -\cos^2 2\omega\tan 2\alpha\sin\beta\,.\,d\beta,$$

or

$$d\omega = -\tfrac{1}{4}k\sin 4\omega\tan kz\,.\,dz.$$

This equation gives us the rotation of the plane of polarisation due to the doubly refracting nature of the medium, while the light passes through a thickness dz. Let us suppose the effect of the magnetic rotation on the light traversing the element dz, may be represented by an additional rotation of these axes

$$d\omega = m\,dz,$$

where m is a constant depending on the nature of the medium and the strength of the magnetic field. Hence, when both these small effects are superposed we get

$$d\omega = m\,dz - \tfrac{1}{4}k\sin 4\omega\tan kz\,dz.$$

Let us denote by ω_1 the value of ω when m is positive, and ω_2 its value when m is negative. Then the apparent magnetic rotation is $\omega_1-\omega_2$, Ω say.

We have

$$d\Omega = 2m\,dz - \tfrac{1}{2}k\,.\sin 2\Omega\cos 2(\omega_1+\omega_2)\tan kz\,dz.$$

This equation is easily integrated when Ω is small. In that case

we may write 2Ω for $\sin 2\Omega$, and $\cos 4\omega_0$ for $\cos 2(\omega_1+\omega_2)$, where ω_0 is the value of ω when $m = 0$. Since

$$\tan 2\omega_0 = \tan 2\alpha \cos kz,$$

we have, putting a equal to $\tan 2\alpha$,

$$\cos 4\omega_0 = \frac{1-a^2\cos^2 kz}{1+a^2\cos^2 kz}.$$

Making these substitutions the differential equation becomes

$$d\Omega = 2m dz - k\Omega \frac{1-a^2\cos^2 kz}{1+a^2\cos^2 kz} \tan kz \,.\, dz,$$

or, putting $\beta = kz$,

$$\frac{d\Omega}{d\beta} = \frac{2m}{k} - \Omega \frac{1-a^2\cos^2\beta}{1+a^2\cos^2\beta}\tan\beta.$$

Put $\mathrm{P} = \dfrac{1-a^2\cos^2\beta}{1+a^2\cos^2\beta}\tan\beta$ for brevity, and the integral of the equation becomes

$$\Omega e^{\int \mathrm{P}d\beta} = \frac{2m}{k} \cdot \int_0^\beta e^{\int \mathrm{P}d\beta} d\beta.$$

It is easily shown that

$$\int \mathrm{P}d\beta = \log \frac{1+a^2\cos^2\beta}{\cos\beta}$$

$$\therefore e^{\int \mathrm{P}d\beta} = \sec\beta + a^2\cos\beta.$$

Substituting we find

$$\Omega = \frac{2m}{k} \cdot \frac{\int_0^\beta (\sec\beta + a^2\cos\beta)\,d\beta}{\sec\beta + a^2\cos\beta}$$

$$= \frac{2m}{k} \frac{\log_e \tan(\tfrac{1}{4}\pi + \tfrac{1}{2}kz) + a^2 \sin kz}{\sec kz + a^2\cos kz}.$$

It appears from this equation that Ω is a periodic function of z, the wave-length being π/k.

If we make k very small, we may put $\log\tan(\tfrac{1}{4}\pi + \tfrac{1}{2}kz) = kz$, and the equation becomes

$$\Omega = \frac{2m}{k} \frac{kz + a^2 kz}{1+a^2}$$

$$= 2mz,$$

an equation which gives the true magnetic rotation, as of course it should do.

If $\alpha = \pi/4$, so that a becomes infinite, the equation becomes

$$\Omega = \frac{2m}{k}\tan kz,$$

except when $kz = \pi/2$. If $kz = \pi/2$, Ω takes an indeterminate form. In this case the light is circularly polarised.

To examine this equation generally it is advisable to write it somewhat differently. Let us put

$$\Omega = \frac{2mz}{kz}\,\frac{\log\tan(\frac{1}{4}\pi + \frac{1}{2}kz) + a^2\sin kz}{\sec kz + a^2\cos kz}$$

$$= \frac{\Theta}{\beta}f(a^2, \beta)$$

where Θ is the true magnetic rotation, and β is the total retardation expressed as an angle.

Hence $$\frac{\Omega}{\Theta} = \frac{f(a,^2\beta)}{\beta}.$$

If then we trace the curve

$$y = f(a,^2x),$$

the ratio of y/x at any point gives us the ratio of the apparent to the true rotation when $\beta = x$.

With regard to the curve,

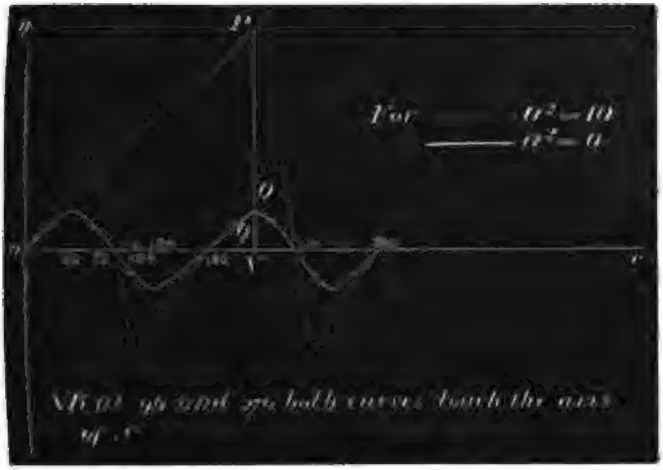
$$y = f(a,^2x),$$

it should be observed that $y = 0$ whenever x is a multiple of $\frac{1}{2}\pi$, and that $\frac{dy}{dx} = 1$ when x is an even multiple of $\frac{1}{2}\pi$, and $= 0$ when x is an odd multiple of $\frac{1}{2}\pi$. Since all along the tangent at the origin $y = x$, we may take the tangent to represent the true rotation, so that

$$\frac{y_{\text{curve}}}{y_{\text{tangent}}} = \frac{\Omega}{\Theta}.$$

Tracings of the curve when $a^2 = 0$ and 10 respectively are given below.

It appears from these curves that when $a^2 = 10$, the apparent rotation is greater than the real rotation if β is less than 80°, while if a^2 is equal to 0, Ω/Θ is always less than 1. When β is small it is easy



to see that the value of Ω/Θ is greater or less than unity, according as a is greater or less than unity. For if β is small,

$$\frac{\Omega}{\Theta}=\frac{\beta(1+a^2)}{\beta(\sec\beta+a^2\cos\beta)}$$

$$=\frac{1+a^2}{1+a^2+(\beta^2/2)(1-a^2)},$$

a fraction which is greater or less than 1 according as $1-a^2$ is negative or positive. The curve tracings also show that the apparent rotation changes sign whenever β is any multiple of a right angle.

[If β is greater than 90°, equal say to $n\,90°+\gamma$, where γ is less than a right angle, then if n is even,

$$\frac{\Omega}{\Theta}=\frac{f(a^2,\gamma)}{\beta};$$

and if n is odd,

$$\frac{\Omega}{\Theta}=\frac{f(a^2,\,90°-\gamma)}{\beta},$$

quantities which become very small when β is large.—May 14.]

In the case of quartz one centimetre thick, where the direction of the ray of light makes an angle of 90° with the optic axis, $\beta=627\,\pi/2$ very nearly. Hence Ω/Θ in this case is about $\frac{1}{627}$ at best, which gives a value of Ω quite inappreciable by any known methods. To find the true magnetic rotation in quartz we must use thin sections about 0·01 mm., and a^2 should be equal to 1.

This value for Ω/Θ accounts for the fact that doubly refracting bodies do not exhibit any rotation of the plane of polarisation,* if the direction of the ray of light is inclined to the optic axis. When light goes through a crystal in the direction of the optic axis, magnetic rotation has been observed. E. Becquerel has observed rotation in tourmalin and rock crystal, while Wertheim† has detected a well-marked rotation in beryl, a feeble one in quartz, but nothing in Iceland spar.

[Lüdtge‡ determined the magnetic rotation along the optic axis of quartz and at various inclinations. The following table gives his results and the corresponding values of β:—

Inclination to optic axis	0°	1°	2°	3°	5°
Rotation (Ω)	1·1	1	0·9	0·6	0·4
Retardation (β)	0	13	52	117	325

Lüdtge adds that these figures are not to be taken as giving exact measurements. No magnetic rotation has been observed in Iceland spar even along the optic axis, and it is worthy of note that in this crystal β very rapidly increases with the inclination to the optic axis. If the length of the spar be 1 centimetre, then at an inclination of $n°$ to the optic axis for D line $\beta = 318° \times n^2$, while in quartz $\beta = 17° \times n^2$.—May 14.]

Wertheim has also shown that if a piece of heavy glass be compressed, the magnetic rotation is diminished, even when the retardation due to the doubly refracting nature of the compressed glass is much less than a wave-length.

Lüdtge's experiments on compressed glass show again how the magnetic rotation is diminished as the doubly refracting property increases. Lüdtge gives the following table, where n represents part of a wave-length retardation and d the magnetic rotation:—

n....	0	0·01	0·2	0·25	0·3	0·45	0·5	0·6
d....	5°	4·6°	4·2°	4°	3·7°	3·5°	3°	2·4°.

These results cannot be directly compared with what we should expect from a crystal, since the ends of the glass are free from strain, and a rotation is there produced, which is observed.

Villari's results are very similar to Lüdtge's. Villari, by spinning a disk of glass very rapidly, strained it, and on observing the magnetic rotation found it get less and less as the strain got greater and greater. There is, however, one noticeable difference between

* Wiedemann, 'Die Lehre von der Elektricität,' vol. 3, sec. 1097.

† Wertheim, 'Compt. Rend.,' vol. 32.

‡ Lüdtge, 'Poggendorff, Annalen,' vol. 137.

Villari's strained disk and Lüdtge's strained prism. The disk was free from strain in the middle, the prism free from strain at the ends.

I have repeated Villari's experiment at the Cavendish Laboratory, using, at Mr. Glazebrook's suggestion, an elliptic analyser to determine the magnetic rotation. With the disk spinning about 200 times a second, the magnetic rotation was reduced from 10° to 6°. This is not so great a diminution as Villari observed, but his glass may have been softer and more easily strained.

Villari thought that the effect he observed was due to the time required to magnetise the glass. That this supposition was erroneous has been clearly established by the experiments of Bichat and Blondlot, recently repeated by Dr. Lodge. In these experiments the oscillating discharge of a Leyden jar was found to rotate the plane of polarisation in time with the oscillations. Before hearing of these results I had myself attacked the problem in a somewhat similar manner. A coil of wire was wound round a piece of heavy glass, and a current alternated 250 times a second by a tuning-fork was sent through the coil. The current was measured by a dynamometer and a tangent galvanometer. The first gave the measure of the current independently of its sign, the second showed that the integral current was zero. When the current was passing it was found impossible to extinguish the light, owing to the rapid alternations of the plane of polarisation.

In conclusion, I have to express my thanks to Professor Thomson and Mr. Glazebrook for many kind suggestions and encouragement, and especially to Professor Thomson for the privilege of using the Cavendish Laboratory.

II. "Revision of the Atomic Weight of Gold." By J. W. MALLET, F.R.S., Professor of Chemistry in the University of Virginia. Received April 15, 1889.

(Abstract.)

After noticing and giving the results of the earlier determinations of the atomic weight of gold, and the recent researches of Krüss and of Thorpe and Laurie, the author reports upon experiments of his own in the same direction, which have occupied much of his time and labour for the last three or four years.

The difficulties connected with the accurate determination of the atomic weight of this metal are remarked upon, and the general principles are reviewed which ought to be observed in all investigations of this kind.

The means and methods of weighing used are stated, and the pre-

cautions are described in detail which were resorted to in purifying the metallic gold to be employed in the research, with the history of some samples of "proof" and "trial plate" gold, obtained from the Mint establishments of the United States and England. The general precautions observed in the course of the work are described, particularly the use of an arrangement for evaporating some of the gold solutions with exclusion of the organic matter of atmospheric dust.

A detailed account is given of the methods adopted in seven series of experiments, looking to more or less independent determinations of the atomic weight sought, viz.:—

First Series.—Division of a uniform neutral solution in water of auric chloride into two accurately weighed and nearly equal portions; precipitation from the one portion by sulphur dioxide of metallic gold, which was collected and weighed; determination, with special precautions, of the quantity of metallic silver required, as nitrate, to precipitate the chlorine in the other portion.

Second Series.—Similar treatment of a neutral solution of auric bromide, giving the quantity of gold in one portion, and the quantity of silver required to precipitate bromine in the other portion.

Third Series.—Similar treatment of a solution of repeatedly crystallised potassium auri-bromide, again giving the quantity of gold in one portion, and the silver equivalent to the whole of the bromine in the other portion.

Fourth Series.—Determination of the loss by ignition of a weighed quantity of trimethyl-ammonium auri-chloride.

Fifth Series.—Comparison of the weights of gold and silver simultaneously deposited by the same electric current from aqueous solutions of auro-cyanide and argento-cyanide of potassium respectively.

Sixth Series.—Comparison of the weight of gold deposited from a solution of potassium auro-cyanide on electrolysis with the volume of hydrogen liberated by the same current from dilute sulphuric acid, the hydrogen evolved in a voltameter of special construction, and measured of course under well-defined conditions of temperature and pressure.

Seventh Series.—Determination of the volume of hydrogen, under known conditions of temperature and pressure, obtainable by solution in dilute sulphuric acid of a given quantity of specially purified metallic zinc; use of a definite quantity of the same zinc, taken in small excess, to precipitate gold from a neutral solution of auric chloride, and determination of the quantity of metallic gold thrown down; determination of the volume of hydrogen obtainable on solution in dilute sulphuric acid of the *excess* of zinc thus used. Resulting comparison of the quantity of gold in solution as auric chloride with the quantity of hydrogen equivalent to the metal.

The results obtained are then stated as follows:—

	Atomic weight of gold.		
	Average value from aggregate weights.	Lowest value from a single experiment.	Highest value from a single experiment.
First series (5 experiments)....	196·722	196·688	196·770
Second " (6 ")....	196·790	196·731	196·843
Third " (4 ")....	196·775	196·685	196·817
Fourth " (5 ")....	197·225	197·131	197·289
Fifth " (5 ")....	196·823	196·709	196·945
Sixth " (3 ")....	197·137	196·994	197·283
Seventh " (6 ")....	196·897	196·848	196·956

If the general mean be taken of the results of all these series of experiments, using the average value derived from each, and giving all an equal weight, the number 196·910 is obtained for the atomic weight of gold.

But reasons are given for feeling much less confidence in the results of the fifth and sixth series of experiments (made by electrolysis) than in the rest; if these two series be excluded the general mean becomes 196·882.

A certain degree of suspicion as to possible constant error having been shown to perhaps affect the results of the fourth series, if this also be left out, and only the first three and the seventh series be considered, the general mean will be 196·796.

And finally, if, for the sake of comparison with the results of the recent researches of other chemists, only the first three series be included, in which auric chloride and bromide were examined, the general mean will be 196·762—a result rather higher than that of Krüss and lower than that of Thorpe and Laurie, but nearer to the latter than the former.

In conclusion, reference is made to the bearing of the results reached on Mendelejeff's periodic classification of the elements, and on the hypothesis of Prout, and attention is drawn to the desirability of a general re-examination of atomic weights, not by a single method only in each case, or by methods more or less nearly similar or dependent on each other, resting satisfied with a close agreement of results under these conditions, but by as many distinct and independent methods as may be possible for each element examined.

III. "Zirconium and its Atomic Weight." By G. H. BAILEY, D.Sc., Ph.D., The Owens College. Communicated by Sir HENRY ROSCOE, V.P.R.S. Received April 15, 1889.

The investigation described in the following paper was undertaken primarily with a view to the redetermination of the atomic weight of zirconium. In previous determinations the sulphate had been used by Berzelius ('Poggendorff, Annalen,' vol. 4, p. 126) and Mats Weibull ("Om Zirkonium och dess föreningar"), the chloride and oxychloride by Hermann ('Journ. Prakt. Chem.,' vol. 31, p. 77), and the double fluoride of potassium and zirconium by Marignac ('Annales de Chimie,' vol. 60, 1860, p. 270). The results of these determinations are summed up in the following table, the relation O : H = 15·96 : 1 being used in the calculations.

	Mean determination.	Maximum.	Minimum.	Author.
$Zr(SO_4)_2$: ZrO_2......	89·23	89·42	89·05	Berzelius.
$Zr(SO_4)_2$: ZrO_2......	89·255	90·158	88·97	Mats Weibull.
Zr : Cl_4........	88·56	..	..	Hermann.
ZrO_2 : HCl.......	89·915	90·76	89·07	"
K_2ZrF_6 : K_2SO_4	90·31	92·57	89·84	Marignac.
ZrO_2 : K_2SO_4.....	90·47	90·97	90·02	"
K_2ZrF_6 : ZrO_2......	90·6	91·1	89·7	"

The sulphate is somewhat readily decomposed by heat, the chloride and oxychloride are known to be unstable, and liable to considerable variation of composition according to the method of preparations, and the conditions under which these variations of composition are brought about were not understood, except in so far as the researches of Troost and others have indicated the great tendency which they show to pass into volatile and non-volatile oxychlorides of complicated character.

The determination is rendered difficult and unsatisfactory in consequence of—

(*a.*) The difficulty of obtaining pure zirconia, and its separation from iron, titanium, and from silica.

(*b.*) The tendency to form basic salts, and the hygroscopic character of its salts, and their general instability.

(*c.*) The tenacity with which the hydrated zirconia retains the alkalies and smaller quantities of impurities in general.

Under these circumstances it seemed to me essential that any determination of the atomic weight of this element should be preceded by an inquiry into the best method of preparing pure zirconia, and an examination of the salts which might be made use of for the purpose, especially with reference to their stability.

Preparation of Crude Zirconia.

The raw material consisted of about 300 grams of zircons from North Carolina, for which I was indebted to the kindness of Sir Henry Roscoe, and in addition to this a quantity of impure zirconia. The zircons were powdered by heating them to redness, and then throwing them quickly into cold water; they were then roughly powdered in an iron mortar, and finally in an agate mortar, and passed through linen. Part of this was treated with hydrogen potassium fluoride, the product being extracted with water and recrystallised many times from hot water. Prepared in this way, however, the zirconia was still impure, and showed the presence of iron in small quantities, and in addition to this it was extremely difficult to get rid of alkali.

The methods of fusion with sodium carbonate or acid potassium sulphate are troublesome and even less satisfactory.

Ultimately caustic soda and sodium fluoride were adopted for breaking up the zircons as recommended by Linnemann, and a modified form of the process followed out by him ('Monatsh. f. Chem.,' vol. 6, p. 337) was adopted as the most convenient. A mixture of 100 grams of caustic soda and 10 grams of sodium fluoride is heated in a covered silver dish over a large Bunsen burner, 25 grams of finely powdered zircons are added in portions, the melt being kept stirred, and the heating continued for about an hour or so long as there is any effervescence. The resulting mass is broken up and extracted with water, the residue consisting principally of sodium zirconate, but containing also silica and undecomposed zircon, oxide of iron, &c. This is now digested with dilute hydrochloric acid and filtered, the residue being subjected repeatedly to the same treatment. The acid solution containing the zirconium is evaporated to perfect dryness, and then exhausted with very dilute hydrochloric acid, and the silica filtered off. The process of solution and evaporation is repeated until the residue obtained dissolves completely in the acid. The impurity now present in the largest quantity is iron, and it was thought desirable to remove the bulk of this first. The hydrochloric acid solution is, therefore, evaporated to small bulk, and left to cool; the oxychloride of zirconium separates out in acicular crystals, leaving the iron in solution. The crystals are thrown into a funnel, and the liquid as far as possible removed at the pump, and the process of solution and separation of crystals in this way repeated several

times. On precipitation of the final products with ammonia the crude zirconia is obtained, which forms the starting point for further purification. In further operations, reagents (such as potassium or sodium salts) which could not be removed by volatilisation were avoided, and even at the present stage it was considered advisable to redissolve the zirconia in hydrochloric acid, and reprecipitate with ammonia three times in order to get rid of the bulk of the sodium salts.

Purification of the Crude Zirconia.

This crude zirconia, free from the impurities that occur in larger quantity, was dissolved in hydrochloric acid, and crystallised out as oxychloride. The aqueous solution of this, slightly acidulated, was treated with sulphuretted hydrogen for twenty-four hours; a gelatinous precipitate was obtained of a purplish-black colour, consisting principally of sulphides of silver and copper, together with some hydrated zirconia. To the filtrate saturated with sulphuretted hydrogen was now added a little ammonia, which had the effect of bringing down a little more zirconia, and also the small quantities of impurities precipitable by ammonium sulphide. The liquid was now acidulated and boiled down to get rid of the sulphuretted hydrogen, and excess of oxalic acid added to the concentrated solution to precipitate gadolinite earths, and finally treated with ammonium oxalate. There still remained some iron which had either not been removed or had been introduced in the course of treatment. To separate this the oxychloride was crystallised repeatedly from concentrated hydrochloric acid, and finally washed with a mixture of the acid with one part of alcohol and ten parts of ether. The only impurity that could now be detected was soda, which the zirconia retained with the greatest tenacity.

This was removed by precipitating the zirconia several times by means of ammonia, and redissolving it in hydrochloric acid. The zirconia finally precipitated by ammonia was washed* so long as any ammonium salts could be detected in the wash water.

The zirconia obtained was tested as to purity in the following way:—

(1.) A portion treated with hydrofluoric acid showed no alteration in weight;

(2.) The moist oxide dissolved completely in oxalic acid;

(3.) On ignition it was perfectly white (inclined to a bluish cast), and a quantity exhausted with hydrochloric showed no trace of iron by the most delicate tests.

* Some of the earlier results had to be rejected because of incomplete washing. 20 grams of zirconia required to be washed 150 times at the pump before ammonia salts were got rid of, at least 50 litres of water being used in the process.

In order, however, to have a further guarantee that the product used for the determination of atomic weight had a definite composition and character, the experiments described later for the determination of atomic weight were made upon four portions, whose final treatment was as essentially different as possible.

Portion A was precipitated from an acid solution of the sulphate by the addition of hydrogen peroxide containing no other impurity than sulphuric acid.

Portion B was transformed into the tetrachloride by heating the zirconia with charcoal in a current of chlorine and then reprecipitated by ammonia.

Portion C was crystallised out from concentrated sulphuric acid by boiling off part of the excess of acid and then allowing to cool.

Portion D was further recrystallised from hydrochloric acid and washed very thoroughly with a mixture of hydrochloric acid, alcohol, and ether as described.

Stability of the Zirconium Salts.

In the case of zirconium it was evident that the number of compounds of a simple character and of sufficient stability to afford a good basis for a determination of atomic weight must be very limited. The principles, therefore, on which it was decided to proceed were—

(*a.*) To prepare zirconia by several independent methods.

(*b.*) To ascertain the most stable salt (or salts if more than one should prove stable), and the most simple transformation of this which would lead to an accurate determination.

Indeed it seems to me in general that unless there are two or more salts of a very stable character, that reliance should lean rather towards the production of the raw material by independent processes, than towards the confirmation of results obtained from a more stable salt by means of values derived from observations on one that is less stable.

The methods previously used in the determination involved—

(1.) Relation of zirconium sulphate to oxide.

(*a.*) Direct conversion by ignition.

(*b.*) *Viâ* $BaSO_4$ by precipitation.

Mats Weibull (*loc. cit.*); Berzelius ('Pogg. Ann.,' vol. 4, p. 126).

(2.) Relation of Zr (in ZrO_2) to Cl (in AgCl).

(*a.*) By analysis of $ZrCl_4$.

(*b.*) By analysis of $ZrOCl_2$.

Hermann ('Journ. Pr. Chem.,' vol. 97, p. 323).

(3.) Relation of K_2ZrF_6 to ZrO_2 and to K_2SO_4. Marignac ('Ann. de Chim.,' vol. 60, p. 257).

In addition to these it seemed possible that the relations—

(4.) Zr to ZrO_2 by direct oxidation of the metal,

(5.) Zr (in ZrO_2) to Br (in AgBr) on analysis of $ZrBr_4$, might be worthy of examination.

The bodies, therefore, to be investigated were the metal, the oxide, the chloride, the oxychloride, bromide, sulphate, and double fluoride. With regard to the last mentioned, no further investigation was considered necessary after the very complete treatment it has received at the hands of Marignac following upon the work of Berzelius on the double fluorides.

Preparation and Properties of Zirconium.

Zirconium was first obtained by Berzelius ('Poggendorff, Annalen,' vol. 4, p. 117) by heating the anhydrous potassium zirconium fluoride with metallic potassium in an iron tube. It is described by him as being an amorphous black powder without lustre and resembling charcoal in appearance. Becquerel, by electrolysis of a concentrated solution of the oxychloride, and Troost ('Journ. Prakt. Chem.,' vol. 97, p. 171), by fusing together potassium zirconium fluoride and aluminium, obtained it in a crystalline form, though it does not seem to have been in either case quite pure. Phipson ('Journ. Prakt. Chem.,' vol. 96, p. 447) found that like carbon, boron, and silicon it can be obtained by heating together magnesium and zirconia and then extracting the excess of magnesium and the magnesia by means of dilute hydrochloric acid.

There was left a black amorphous powder resembling that described by Berzelius. No determinations are, however, given to show how far this was free from impurity, and whether it contained unreduced oxide. A repetition of Phipson's experiments showed that as far as the presence of foreign impurity is concerned it leaves nothing to be desired, though I have not succeeded in preparing the metal by this or any other of the methods in a state of sufficient purity for a determination of atomic weight. Phipson's method is a very convenient one for preparing the metal, and the following results will show the conditions most favourable to its production in this way.

Experiments were first tried with intimate mixtures of magnesium powder, 3 grams and 5 grams respectively, with 4·4 grams of zirconia, the mixture being heated to bright redness in an iron tube, the mouth of the tube being filled with finely powdered common salt. There remained, after repeated digestion with dilute hydrochloric acid, a fine black powder, so fine that it passed through the filter-paper and remained in part suspended for weeks in water, showing a reddish-purple coloration by transmitted light.

When dried and heated in air to a point far below redness it glowed and was transformed into zirconia with a small increase in weight. It evidently consisted of zirconia containing only a small percentage of the metal. When rubbed in an agate mortar, however, it showed no white particles, and the variability of its composition, as shown in these and other experiments, lent no support to the assumption that a lower oxide had been formed. In a second set of experiments, magnesium foil was used, and in this case the foil retained its form, although permeated by metallic zirconium exactly as it does in the reduction of carbon compounds. By separating the foil from the finer powder it was possible to obtain zirconium almost free from oxide. Three specimens were prepared: A resembled those already described, a velvety black powder, B had a slight greenish cast, and C a decided olive-green colour with a slight lustrous appearance.

It was thought, therefore, that A would prove to be the purest zirconium, judging from the experience of previous experimenters.

Samples were carefully heated in a stream of air, the moisture and carbonic acid (arising from carbon in the magnesium used) directly determined, and the proportion of Zr and ZrO_2 arrived at by calculation, knowing the original weight of substance taken and the weight of ZrO_2 left in the boat.

A simply glowed and passed into a white powder as the previous specimens had done, B burnt with a white light much more brightly than A had done, and C gave out a dazzling light resembling in its brightness that of burning magnesium; when thrown into the flame of a Bunsen burner they gave beautiful white scintillations.

The results of the analysis gave the following composition:—

	A.	B.	C.
Metallic zirconium .	0·0453	0·1516	0·3383
Zirconia	0·3434	0·3561	0·0267
Moisture	0·0100	0·0086	0·0350
Carbon	0·0015	0·0013	nil

A careful examination showed that no iron or magnesium was present, and, indeed, after precipitation of the zirconia, the residual impurity from half a gram weighed less than a milligram, and consisted entirely of silica, introduced doubtless by the ammonia used in precipitation. Preparation C was, therefore, almost pure zirconium, containing only a slight admixture of oxide; rubbed up in an agate mortar it showed a brassy lustre. It is stated generally that zirconium is hardly acted upon at all by mineral acids or aqua regia, but dissolves in hydrofluoric acid. My own observations confirm these statements, with the exception that concentrated sulphuric acid begins to act in the cold even, and on gently warming the action pro-

ceeds rapidly with a copious evolution of sulphur dioxide, and even with the production of free sulphur.

Dilute sulphuric acid (1 : 1) acts more slowly with the evolution of hydrogen.

Zirconium is acted upon also by chlorine and bromine, in which, on gentle heating, it undergoes vivid combustion, forming the tetrahaloid derivatives, and this is, indeed, a convenient method for obtaining these bodies. The iodide could not be obtained. When the zirconium was filtered, after the digestion with dilute hydrochloric acid, and dried, it was found to have occluded hydrogen. This was removed by exposing it *in vacuo* for some days, and removing the hydrogen by the mercury pump.

The Oxides.—The only points for remark in addition to what has already been published, relate to the reduction (?) of zirconia and to the peroxide of zirconium.

With regard to the former it has already been noticed that when zirconia is heated, surrounded by the reducing flame of coal-gas, it blackens. This blackening is only superficial, however, and is in all probability due to a thin deposit of carbon, and it reoxidises readily to a white powder when heated in air, with a *decrease* of weight—small, indeed, but distinct. Heated at the highest temperature of the blowpipe flame in hydrogen, it undergoes no reduction.

Zirconium Peroxide, ZrO_3.

Clève ('Paris Soc. Chim. Bull.,' vol. 43, p. 53) precipitated an oxide, to which he assigned the composition ZrO_3, by adding ammonia and hydrogen peroxide to a solution containing a zirconium salt; my own experiments ('Chem. Soc. Journ.,' vol. 49, p. 481), in which the oxide was precipitated from a dilute solution by the addition of hydrogen peroxide alone, the solution being slightly acid, gave an oxide agreeing well with the composition Zr_2O_5; it was found also that an oxide resembling Clève's could be obtained without the addition of ammonia. In order to see whether the peroxide, ZrO_3, was identical with that obtained by Clève, I precipitated a large quantity of a moderately concentrated solution of zirconium sulphate, containing a slight excess of acid—

(*a.*) By the addition of hydrogen peroxide alone.

(*b.*) By the addition of hydrogen peroxide, and then ammonia until just alkaline.

	Mean determinations.		Calculated for ZrO_3.
	(*a.*)	(*b*).	
Zr	64·92	64·97	65·21
O	35·08	35·03	34·79

It is evident, therefore, that with due precaution an oxide agreeing very definitely with the formula ZrO_3, is obtainable by precipitation with hydrogen peroxide, either in alkaline or acid solution. The addition of ammonia, especially in excess, is, however, not to be recommended, since it tends to bring down the ordinary oxide, and to decompose the hydrogen peroxide added, with the production of nitrous and nitric acid. Dried for several weeks over phosphorus pentoxide, the oxide became constant in weight and showed the composition $ZrO_3.3H_2O$, the hydrate of zirconia being $ZrO_2.2H_2O$. The salt, however, lost about 2 per cent. of oxygen during the process of drying, and if this was carried out at 100° it had the composition Zr_2O_5.

Zirconium Tetrachloride.

About 30 grams of this salt were prepared in the usual way, by passing dry chlorine over a mixture of charcoal and zirconia. If a determination was to be made from this salt some process of purification was necessary, and a criterion that a body of constant composition had been obtained, especially as it is known that volatile oxychlorides exist.

For this purpose a combustion-tube, in which the chloride was prepared, was drawn out into a series of bulbs, separated by constrictions. The chloride was then sublimed (a lower temperature being used for each successive bulb) in a current of dry chlorine into these bulbs, which were separately sealed off and their contents analysed. It was hoped in this way that two or more bulbs would be obtained showing concordant results, but notwithstanding that every precaution was taken to exclude air and moisture, no such result could be achieved, and decomposition was clearly indicated to have taken place with the formation of traces of a reddish-brown product, the equivalent of chlorine in relation to zirconium being in some cases higher and in others lower than four to one. Furthermore, in no case was it found possible to prepare such a product perfectly free from silica or iron. It seems, indeed, that zirconium tetrachloride is even more liable to dissociation and decomposition than the corresponding silicon compound.

Zirconium Oxychloride.

This salt is prepared, as already stated, by crystallisation from concentrated hydrochloric acid. The difficulties attending its preparation in the pure state arise from the presence of free hydrochloric acid and from the readiness with which the salt passes into oxychlorides of a more basic character. Berzelius attempted to remove the excess of free acid by heating the salt to 60°, but was not able to obtain a definite product, his numbers being—

ZrO_2	0·332	0·485
AgCl	0·661	1·096

whereas the silver chloride should be about $2\frac{1}{3}$ times the amount of the ZrO_2. Paijkull ('Paris Soc. Chim. Bull.,' vol. 20, p. 65) dried the salt between filter-paper, and found the composition of the crystals to be $ZrOCl_2.8H_2O$, the amorphous form precipitated by excess of HCl being $2ZrOCl_2.13H_2O$.

Basic oxychlorides, $Zr_3O_4Cl_4$ (Endemann), $Zr_2O_3Cl_2$, Zr_2OCl_6 (Troost and Hautefeuille), ZrOClOH, $Zr_8O_8Cl_7(OH)_9$ (Endemann), have also been described.

The salt examined by me was prepared by repeated crystallisation from concentrated hydrochloric acid, washed with hydrochloric acid till the washings were colourless and showed no trace of iron, and then the free acid removed—

(*a*.) By washing with a mixture of 1 part of alcohol and 10 parts of ether.

(*b*.) By gently heating the salt.

(*c*.) By exposing the finely powdered salt at ordinary temperature in a vacuous desiccator over potash, until no HCl appeared when air was passed over it.

The analysis was performed by dissolving the salt in water and precipitating the zirconia with ammonia, then acidulating with nitric acid, and precipitating the chlorine by means of silver nitrate.

In the following tables are given Berzelius's determinations and those obtained by using the methods (*a*) and (*c*).

(*b*) is omitted, since it was found that a constant and progressive diminution of chlorine occurred during the process. It will be sufficient for the purpose to state simply the relation of ZrO_2 to AgCl obtained.

	ZrO_2.	:	AgCl.
(1) Berzelius's determination............	1	:	1·991
" "	1	:	2·260
(*a*) Washed with ether and alcohol.......	1	:	2·206
	1	:	2·179
	1	:	2·226
	1	:	2·260
(*c*) Exposed *in vacuo* over KHO..........	1	:	2·264
Without washing	1	:	2·245
	1	:	2·309
	1	:	2·285
$ZrOCl_2(Zr = 90)$ calculated	1	:	2·350

The whole of the specimens analysed dissolved in water to a clear solution, with a slightly acid reaction. The determination which

shows the highest proportion of chlorine would give the atomic weight of zirconium as 92, which is undoubtedly too high, and, moreover, it will be quite evident from these numbers that no trustworthy determination was to be expected from this salt.

The Tetrabromide.

This salt was prepared by heating zirconia in an atmosphere of bromine vapour, but proved no more promising in its behaviour than the corresponding chlorine derivative.

The Sulphate.

Several methods of preparing the sulphate were tried, but ultimately that originally used by Berzelius was adopted. Finely powdered zirconia (air-dried) was heated with concentrated sulphuric acid, and most of the excess of acid driven off at as low a temperature as possible. In order to obtain the normal sulphate Berzelius drove off the excess of sulphuric acid, and then heated for a quarter of an hour, but never to redness; Mats Weibull heated till constant at 300°, a temperature manifestly too low. Clève, in a private communication, advised me to heat in a sulphur bath (442°). It seemed, however, that in this instance, where the atomic weight depended on determinations from the sulphate alone, and especially since this is a general method applicable to several other elements, it was desirable that the limits of temperature within which the normal sulphate was stable ought to be ascertained. This was done in the case of a number of sulphates (Bi, Mg, Zn, Di), and the details of the investigation have already appeared ('Chem. Soc. Journ.,' vol. 51, p. 676).

The salt containing excess of sulphuric acid was heated in a bath, described in that communication, which could be easily kept within 5°, and at any temperature up to 500°. The boat containing the salt was weighed from time to time until it became nearly constant. The salt was then finely powdered, and the heating continued in periods of about four hours, until no further diminution of weight occurred, showing that the free sulphuric acid had been got rid of. The temperature of the bath was now raised by intervals of about 10°, and the heating continued for several hours at each limit, weighings being made after each increment until a point was arrived at at which loss of weight was again observed. This indicated that the temperature had been attained at which the normal salt began to undergo decomposition. We have then the limits of temperature within which the normal sulphate is stable. Several series of determinations were made, and one of these is given to show the character of the values obtained.

				Grams.
Weighing tube and boat				20·64962
"		"	and salt	22·9095
After heating	6	hours at	300°	22·9045
"	7	"	330	22·9015
"	4	"	350	22·9002
"	4	"	365	22·90012
"	5	"	380	22·90025
"	4	"	390	22·90020
"	4	"	410	22·89820

The above numbers have not had applied to them the small corrections for variations of barometer and thermometer, and no importance need be attached to the differences of one or two tenths of a milligramme between 350° and 400°.

It appears, therefore, from these experiments that zirconium sulphate is stable up to 400°, and that the excess of sulphuric acid is driven off completely at 350°. If, therefore, a mixture of the salt and the free acid, prepared as above, be heated at any temperature between 350° and 400° till constant, we shall obtain the normal sulphate. The temperature of the sulphur bath would be too high, and of course at dull redness decomposition would set in.

The Atomic Weight of Zirconium.

After what has been said it may appear that the previous determinations of atomic weight from the sulphate would be too high rather than too low, since there can be little doubt that Berzelius heated the salt above 400°. It does not follow, however, that by heating it for a short time at a moderately high temperature more than a small fraction of the salt would be brought under conditions favourable to decomposition, nor even would the temperature of the mass be such that the whole of the free sulphuric acid would be got rid of. And the fact that on repeating such a treatment no variation in weight occurred, would be no guarantee that the salt was normal zirconium sulphate. My own experiments show that to get rid of the excess of acid requires prolonged heating, and at the same time renewal of the atmosphere in the vessel containing the sulphate. Apropos of this, Berzelius's precaution to introduce into the crucible in the last stage of the ignition of the sulphate a little ammonium carbonate is a very necessary one, as it displaces often 2 or 3 milligrams of sulphuric acid, which otherwise seems to remain in the crucible time after time and fix itself in the zirconia, when the crucible begins to cool. Furthermore, whatever previous experience may have been (for no special precaution is mentioned in any case), I have found it most difficult to ignite zirconium sulphate without loss; the decomposition occurs

with such violence at first, and the resulting zirconia is so extremely light that, though every care was taken, varying results were got, even in igniting very gently over the bunsen burner, some as low as 43 per cent. of oxide instead of 43·3 at least. On carefully watching, it was seen that minute particles of zirconia were being carried out of the crucible, although the cover fitted well. This loss was only avoided by enclosing the crucible within a second larger one, whose cover fitted quite closely and only communicated with the external atmosphere by a drawn-out neck, so that there were no air-currents. Even then, if the crucible was placed over the blowpipe flame at once, loss occurred and it was necessary in every case to commence the operation over the flame of a bunsen burner, and then after half an hour to transfer to the blowpipe.

Part of the determinations were made by Berzelius in the wet way —that is, the zirconia was first precipitated by means of ammonia, and then the sulphuric acid determined in the filtrate by precipitation with barium chloride.

Having satisfied myself by careful comparative experiments that the results of both methods, under favourable conditions, correspond, I decided to adopt the dry method, viz., that of conversion of the sulphate into the oxide by ignition—

(*a*.) Because it involved the least complicated operations and the fewest assumptions.

(*b*.) Because under some circumstances basic zirconium sulphate is thrown down when ammonia is added to a solution of the sulphate.

In the following paragraphs are given the determinations of Berzelius and Mats Weibull from the sulphate, followed by those based upon my own experiments.

Berzelius's Determination ('Pogg. Ann.,' vol. 4, p. 126).

From the analysis of the sulphate:—

SO_3 : ZrO_2	: :	100	: 75·84
,,		,,	: 75·92
,,		,,	: 75·80
,,		,,	: 75·74
,,		,,	: 75·97
,,		,,	: 75·85
Mean			75·853

ZrO_2	: $2SO_3$	: :	1·5171	:	2
Zr	: O	: :	5·591	:	1
Zr	: H	: :	89·23	:	1

Mats Weibull's Determinations.

	$Zr(SO_4)_2$.	ZrO_2.	$\frac{ZrO_2}{Zr(SO_4)_2}$.
	1·5409 grams.	0·6684 gram.	43·126 per cent.
	1·5445 "	0·6665 "	43·153 "
	2·1683 "	0·9360 "	43·168 "
	1·0840 "	0·4670 "	43·081 "
	0·7913 "	0·3422 "	43·321 "
	0·6251 "	0·2695 "	43·113 "
	0·4704 "	0·2027 "	43·091 "
Total ..	8·2335 grams.	3·5523 grams.	43·146 per cent.

Mean determination :—

$$Zr : O :: 5{\cdot}592 : 1$$
$$Zr : H :: 89{\cdot}255 : 1$$

My own Determinations.

	$Zr(SO_4)_2$.	ZrO_2.	$\frac{ZrO_2}{Zr(SO_4)_2}$.
		Portion A.	
	2·02357 grams.	0·87785 gram.	43·381 per cent.
	2·6185 "	1·1354 "	43·360 "
		Portion B.	
	2·27709 grams.	0·98713 gram.	43·350 per cent.
	2·21645 "	0·96152 "	43·385 "
		Portion C.	
	1·75358 grams.	0·76107 gram.	43·4016 per cent.
	1·64065 "	0·7120 "	43·397 "
		Portion D.	
	2·33255 grams.	1·01143 grams.	43·361 per cent.
	1·81105 "	0·78485 "	43·337 "
Totals ..	16·67344 grams.	7·23125 grams.	43·37 per cent.

Maximum	Zr : O ::	5·674 : 1
	Zr : H ::	90·559 : 1
Minimum	Zr : O ::	5·654 : 1
	Zr : H ::	90·237 : 1
Mean	Zr : O ::	5·664 : 1
	Zr : H ::	90·401 : 1

Corrections in the Weighings.

The balance used could be read directly to $\frac{1}{50}$ milligram, and had been proved to be a most reliable instrument. The weights were an excellent set by Staudinger of Giessen, and showed an average variation from the normal amounting to only 0·000035 gram.

Corrections were introduced—

(*a*.) For the weights.
(*b*.) For displacement of air and variations in temperature and pressure.
(*c*.) For variations arising by reason of the different hygroscopic conditions of the desiccator and balance case.

In any case where condensation of moisture was liable to occur this was corrected for by noting the increment of weight per minute for several minutes whilst exposed in the balance case, and then constructing a curve, with increment of weight and time as ordinate and abscissa, from which the necessary correction at the time of weighing could be introduced. In nearly all cases over 2 grams of the sulphate was used for each determination, and even with 2 grams a difference of 1 milligram in the weight of the zirconia obtained implies a difference of 0·25 in the atomic weight; it is, therefore, evident that in some previous determinations, where less than a gram has been taken, there is considerable risk of error in the atomic weight.

IV. "Magnetic and other Physical Properties of Iron at a High Temperature." By JOHN HOPKINSON, F.R.S. Received April 16, 1889.

(Abstract.)

This paper deals with the same subjects as are dealt with in three short papers* already read before the Royal Society. It gives full particulars of the experiments made both on the samples there mentioned and on other samples.

V. "Determining the Strength of Liquids by means of the Voltaic Balance." By G. GORE, LL.D, F.R.S. Received April 17, 1889.

* 1. "Magnetisation of Iron at High Temperatures." (Preliminary Notice.) 'Roy. Soc. Proc.,' vol. 45, p. 318. 2. "Recalescence of Iron." *Ibid.*, p. 455. 3. "Electrical Resistance of Iron at a High Temperature." *Ibid.*, p. 457.

VI. "On Films produced by Vaporised Metals and their Applications to Chemical Analysis.—Preliminary Notice." By W. N. HARTLEY, F.R.S., Royal College of Science, Dublin. Received April 22, 1889.

Having recently communicated to the Royal Dublin Society (March 20th) a paper entitled "On the Constitution of the Electric Spark," I have described a means of obtaining deposits of metals and of metallic oxides, which serves as a very delicate test for some of the metals. It has been considered desirable to devote much further study to the subject, and a method of working has been devised which, so far as it has been applied, appears to be especially useful in the examination of certain metallurgical products, as for instance in the detection and even estimation of the precious metals in copper, lead, and tin, and of certain impurities in copper. The method may be stated to be carried out in the following manner:—Electrodes of the metals to be tested are put into communication with the wires of an induction coil used for the production of condensed sparks, such as serve for the photographing of the ultra-violet spectra. When a plate of mica through which a series of pin-holes has been pricked is placed in the path of the spark and the current is passed for a period varying from 5 to 10, 15, 20, 30, and 60 seconds, a series of deposits of different degrees of tenuity are obtained, which are metallic with the noble metals, such as *silver*, *gold*, *palladium*, *iridium*, and *platinum*, and are oxides with such metals as are oxidisable, for instance *magnesium*, *zinc*, *cadmium*, *lead*, *tin*, *copper*, *iron*, *nickel* and *cobalt*, *aluminium*, *indium*, *thallium*, *arsenic*, *antimony*, and *bismuth*.

Gold gives films of extreme tenuity and beautiful in richness of colour, partly deep red or rose-tinted, but chiefly of a magnificent blue, with a shade of green in the thinnest part of the deposit.

Silver gives yellow films, while *palladium*, *iridium*, and *platinum* give different shades of brown. The films are different as to the area which they cover, the difference being due to the volatility and the colouring power of the metals. Gold is by far the most remarkable in this respect. The colours of the oxides, such as various shades of brown with *iron*, *cobalt*, *nickel*, *thallium*, *cadmium*, *bismuth*, and yellow with *zinc* and *lead*, serve as tests by which they can be recognised. The volatility of the oxides is also a distinctive feature; *arsenic* forms a remarkable series of rings round the pin-hole, and other metals, *antimony* standing next, are thus distinguishable.

It is stated in the paper already quoted that beside the metallic deposit which silver gives, there is a yellow colour and a tinge of rose colour and violet. The silver employed was originally prepared by

the late Dr. W. A. Miller, F.R.S., for spectroscopic purposes, and it is remarkably pure. The photographed spectra of this metal yield no lines traceable to gold or any other foreign element. (For its spectrum see 'Phil. Trans.,' 1884, pp. 109 and 134.) Nevertheless I considered that the rose-red and violet tints on the mica films were due to minute traces of gold. Since then, in order to prove or disprove this point, attempts have been made to prepare specimens of perfectly pure silver by the processes of M. Stas, but so far no samples have been obtained which can be relied upon as absolutely free from gold. In one case the chloride precipitated from a cold solution was digested several times with hot aqua regia, washed completely free from soluble matter until the washings had no acid reaction, and subsequently reduced to metal by boiling with pure caustic alkali and milk sugar. This specimen was found to give a characteristic yellow film, but there was a trace of violet-grey surrounding the yellow of the silver, which resembled the colour obtained from metal to which traces of gold had been added, and which became deeper and deeper in colour as the quantity of gold was increased. An alloy was made by melting this sample of silver and adding thereto $\frac{1}{1000}$th of its weight of pure gold. The alloy was heated to boiling point for a minute; in order to agitate and completely mix the metals, the fused metal was kept in rapid rotation, and it was granulated by pouring it into water. The cooled metal was next treated in a manner intended to render it homogeneous in composition; thus, it was hammered out flat, broken in pieces, and again melted, the fused metal being granulated, and the solidified drops being again hammered into disks. By a precisely similar treatment alloys were made from this metal containing proportions of gold amounting to no more than $\frac{1}{10000}$th and $\frac{1}{100000}$th respectively.

The hammered beads, generally about a decigramme in weight, were submitted to the action of the spark, and deposits of the metal on mica were obtained by passing the spark for 5, 10, 15, 20, 30, and 60 seconds.

The difference between the deposits from the alloy containing $\frac{1}{1000}$th of gold was clearly seen with the naked eye in all cases. The alloy containing $\frac{1}{10000}$th was as clearly seen to contain less gold than the foregoing, and more than that with $\frac{1}{100000}$th; and that with $\frac{1}{100000}$th gave unmistakeable evidence of gold when examined with a 2-inch power under the microscope. The gold tints of rose-red and blue were observed as rings even in cases where the spark had passed for no longer than 5 seconds. Indeed these tests with the shorter duration of the spark gave the best evidence, since too much of the silver deposit obscured the characteristic colours. On the other hand, a much longer exposure of 10 or 20 minutes widely diffused the deposit of gold, and rendered it evident outside the silver. It may

thus far be considered as proved that a decigramme of silver containing $\frac{1}{100000}$th of gold gives a distinctive deposit of this metal, which is recognised by its colour, and that silver as pure as that obtained by the process of M. Stas which I have described gives no such decisive indication, though it does not yield films which can be considered as absolutely free from gold. Furthermore, this test for gold is more sensitive than that depending upon the employment of the spectroscope. Many specimens of copper and silver have been tested, and in each of them there appears to be a trace of gold.

Gold of the fineness of 9, 12, 15, 18, and 22 carats has been shown to yield films each with distinctive characters.

The various deposits of oxides and of some metals are easily treated with such acid and other reagents as are gaseous or capable of being volatilised, and the colour reactions of the sulphides may be obtained as definite rings of larger or smaller diameter, corresponding with the volatility of the metals.

The application of this method may ultimately be made to lead to valuable results, but most of the large number of films already prepared have yet to be examined, and their properties described; it is therefore proposed to deal exhaustively with the subject in a future communication.

Presents, May 9, 1889.

Transactions.

Alnwick:—Berwickshire Naturalists' Club. Proceedings. Vol. XII. Part 1. 8vo. [*Alnwick* 1888.] The Club.

Baltimore:—Johns Hopkins University. Circular. Vol. VIII. No. 72. 4to. *Baltimore* 1889; Studies in Historical and Political Science. Ser. 7. No. 4. 8vo. *Baltimore* 1889; Notes Supplementary to the Studies. Nos. 2–3. 8vo. *Baltimore* 1889. The University.

Berlin:—K. Preuss. Akademie der Wissenschaften. Sitzungsberichte. Jahrgang 1888. Nr. 38–52. 8vo. *Berlin.* The Academy.

Catania:—Accademia Gioenia di Scienze Naturali. Bullettino Mensile 1889. Fasc. 4–5. 8vo. *Catania* 1889. The Academy.

Emden:—Naturforschende Gesellschaft. Jahresbericht 72–73. 8vo. *Emden* 1889. The Society.

Kew:—Royal Gardens. Bulletin of Miscellaneous Information. No. 28. 8vo. *London* 1889. The Director.

Leipsic:—Astronomische Gesellschaft. Vierteljahrsschrift. Jahrgang XXIV. Heft 1. 8vo. *Leipzig* 1889. The Society.

Transactions (*continued*).

Königl. Sächs. Gesellschaft der Wissenschaften. Abhandlungen. Bd. XV. Nr. 3–4. 8vo. *Leipzig* 1889. The Society.

London:—British Association. Report. 1888. 8vo. *London* 1889. The Association.

Entomological Society. Transactions. 1889. Part 1. 8vo. *London.* The Society.

London Mathematical Society. Proceedings. Vol. XX. Nos. 343–345. 8vo. [*London*] 1889. The Society.

Physical Society. Proceedings. Vol. X. Part 1. 8vo. *London* 1889. The Society.

Royal Meteorological Society. Quarterly Journal. Vol. XV. No. 69. 8vo. *London* 1889. The Society.

Royal Microscopical Society. Journal. 1889. Part 2. 8vo. *London.* The Society.

Royal Statistical Society. Journal. Vol. LII. Part 1. 8vo. *London* 1889. General Index. 1873-1887. 8vo. *London* 1889. The Society.

Milan:—Reale Istituto Lombardo di Scienze e Lettere. Rendiconti. Ser. 2. Vol. XX. 8vo. *Milano* 1887. The Institute.

Società Italiana di Scienze Naturali. Atti. Vol. XXX. Fasc. 1–4. 8vo. *Milano* 1887–88. The Society.

New York:—American Geographical Society. Bulletin. Vol. XXI. No. 1. 8vo. *New York* 1889. The Society.

Vienna:—Kaiserliche Akademie der Wissenschaften. Anzeiger. Jahrgang 1889. Nr. 4–8. 8vo. *Wien.* The Academy.

K. K. Geologische Reichsanstalt. Verhandlungen. 1889. No. 3. 8vo. *Wien.* The Institute.

Cameron (Sir C. A.) History of the Royal College of Surgeons in Ireland. 8vo. *Dublin* 1886. The College.

Johnston (R. M.) Systematic Account of the Geology of Tasmania. 4to. *Tasmania* 1888. The Government of Tasmania.

Maiden (J. H.) The Useful Native Plants of Australia. 8vo. *London* 1889. The Author.

Scacchi (A.) Catalogo dei Minerali e delle Rocce Vesuviane. 4to. *Napoli* 1889; Sulle Ossa Fossili trovate nel Tufo dei Vulcani Fluoriferi della Campania. 4to. *Napoli* 1888; La Regione Vulcanica Fluorifera della Campania. 4to. *Napoli* 1887. The Author.

Sclater (P. L.), F.R.S., and W. H. Hudson. Argentine Ornithology. Vol. II. 8vo. *London* 1889. The Authors.

Uslar (Baron P.) Ethnography of the Caucasus. Linguistics III. —The Avar Language [Russian]. 8vo. *Tiflis* 1889. The Author.

May 16, 1889.

Professor G. G. STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On a possible Geological Origin of Terrestrial Magnetism." By EDWARD HULL, M.A., LL.D., F.R.S., Director of the Geological Survey of Ireland. Received April 30, 1889.

(Abstract.)

The author commenced by pointing out that the origin and cause of terrestrial magnetism were still subjects of controversy amongst physicists, and this paper was intended to show that the earth itself contains within its crust a source to which these phenomena may be traced, as hinted at by Gilbert, Biot, and others; though owing to the want of evidence regarding the physical structure of our globe in the time of these observers, they were unable to identify the supposed earth's internal magnet.

The author then proceeded to show cause for believing that there exists beneath the crust an outer and inner envelope or "magma," the former less dense and highly silicated, the latter basic and rich in magnetic iron-ore. This view was in accordance with those of Durocher, Prestwich, Fisher, and many other geologists. The composition of this inner magma, and the condition in which the magnetic iron-ore exists were then discussed, and it was shown that it probably exists under the form of numerous small crystals with a polar arrangement. Each little crystal being itself a magnet, and having crystallised out from the magma while this latter was in a viscous condition, the crystalline grains would necessarily assume a polar arrangement which would be one of equilibrium. Basalt might be taken as the typical rock of this magma.

The thickness and depth of the magnetic magma beneath the surface of the globe were then discussed, and while admitting that it was impossible to come to any close determination on these points owing to our ignorance of the relative effects of increasing temperature and pressure, it was assumed tentatively that the outer surface of the effective magnetic magma might be at an average depth of about 100 miles, and the thickness about 25 or 30 miles. The proportion of mag-

netic iron-ore in basaltic rocks was then considered, and it was shown that an average of 10 to 15 per cent. would express these proportions; and assuming similar proportions to exist in the earth's magnetic magma, we should then have an effective terrestrial magnet of from 2½ to 3 miles in thickness. The actual magnetic magma or shell might be very much thicker than that here assumed.

Instances of polarity in basaltic masses at various localities were adduced in order to illustrate the possibility of polarity in the internal mass. The subject of the polarity of the globe was then discussed, and it was pointed out how the position of the so-called "magnetic poles" leads to the inference that they are in some way dependent upon the position of the terrestrial poles.

The author regarded the so-called "double poles" as merely *foci* due to protuberances of the magnetic magma into the exterior non-magnetic magma, and considered that there was really only a single magnetic pole in each hemisphere, embracing the whole region round the terrestrial pole and the *stronger and weaker magnetic foci*, and roughly included within the latitude of 70° within the northern hemisphere.

It was pointed out that the poles of a bar-magnet embrace a comparatively large area of its surface, and hence a natural terrestrial magnet of the size here indicated may be inferred to embrace a proportionably large tract for its poles.

In reference to the question why the magnetic poles are situated near those of the earth itself, this phenomenon seemed to be connected with the original consolidation of the crust of the globe, and the formation of its internal magmas.

It was pointed out that, owing to the differences of temperature which must have existed in the polar regions, as compared with those of the equatorial, the process of solidification has been more rapid in the polar regions than elsewhere, and it was inferred that in the case of the magnetic magma the process of crystallisation and the polar arrangement of the particles of magnetic iron-ore would proceed from the poles towards the equator in a radial direction. The manner in which the phenomena of magnetic intensity, and of the dip of the needle at different latitudes could be explained on the hypothesis of an earth's internal magnet, such as here described, was then pointed out, and the analogy of such a magnet with a magnetic bar passing through the centre of the earth was illustrated.

The author then proceeded to account on geo-dynamical principles for the secular variation of the magnetic needle, and also to show how the objections that might be raised to the views here advanced, on the grounds of the high temperature which must be assumed to exist at the depth beneath the surface of the magnetic magma, could be met by considerations of pressure, and on this subject read a letter which he had received from Sir William Thomson, F.R.S.

In conclusion, the author stated it was impossible in a short abstract to go into the details of the subjects here discussed, and for further information the reader must be referred to the paper itself.

II. "Physiological Action of the Active Principle of the Seeds of *Abrus precatorius* (Jequirity)." By SIDNEY MARTIN, M.D. London, British Medical Association Research Scholar, Assistant Physician to the Victoria Park Chest Hospital, and R. NORRIS WOLFENDEN, M.D. (Cantab.). (From the Physiological Laboratory, University College.) Communicated by E. A. SCHÄFER, F.R.S. Received April 11, 1889.

The object of the present investigation was to study the physiological action of the active principle of the jequirity seed. A watery infusion of the seeds, as is well known, produces severe inflammation of the conjunctiva when a few drops are placed in the eye; and when injected under the skin, or as in the "sui" poisoning of cattle in India, it is fatal to animals.

Both the local irritant and the poisonous properties of the seed were formerly ascribed to a specific organism, called the jequirity bacillus, the nature of which was investigated by Sattler, Cornil, and Berlioz. Klein, however, showed that the action could not be due to a bacillus, since the poison was permanently destroyed by momentary boiling of the infusion. Warden and Waddell have effectually disposed of the bacillus theory of the action of jequirity, and in a pamphlet entitled the 'Non-Bacillar Nature of Abrus Poison' (Calcutta, 1884), they demonstrated that the poisonous activity of the seeds was dependent on a proteid body which was called by them *Abrin*. Abrin was considered to be closely allied to egg-albumin and the vegetable albumins. The reactions given by Warden and Waddell are, however, by no means conclusive that abrin belongs to the class of "albumins" as understood by physiological chemists. The fact that it is precipitated from solution by acetic acid shows that it is not an albumin: this is a reaction common to globulin and certain other proteids, such as alkali-albumin. The reactions, moreover, given by these observers as given by abrin are not distinctive of it, but are common to all proteids.

To clear up these discrepancies the proteids of the seed were investigated by one of us (M.), and in a paper published in the 'Proceedings of the Royal Society' (vol. 42, p. 331) two proteids were described, a globulin and an albumose. The globulin was found to be vegetable paraglobulin, being soluble in 15 per cent. sodium chloride solution, and coagulating in 10 per cent. magnesium sulphate

solution between 75° and 80° C. The albumose gave the general reactions of its class, and corresponded to what has been called by one of us (M.) α-phytalbumose.*

The physiological action of the globulin has been investigated by us. It was isolated by the method detailed in the paper already quoted. As the method employed is important, it is again mentioned here. Both globulin and albumose were extracted from the decorticated and crushed seed by a 15 per cent. solution of sodium chloride, and the clear extract saturated with sodium chloride or ammonium sulphate after acidulation with acetic acid. The copious saturation precipitate consists of globulin and albumose: it is dissolved in distilled water, and dialysed in running water until the globulin is in great part thrown down. The precipitate of globulin so obtained is separated by filtration and washed on the filter with previously boiled distilled water until no soluble proteid is present in the washings. The distilled water removes the salt clinging to the globulin and the small amount of albumose present. After this thorough washing the globulin which remains on the filter is a pure proteid. It is removed from the filter and dried over sulphuric acid. If it has been precipitated by ammonium sulphate, the globulin dries in greenish-black scales, owing to the admixture of colouring matter; but if sodium chloride has been used, it is obtained as a whitish-yellow amorphous powder. This was the powder used in our experiments. It consists of pure globulin, almost completely soluble in 15 per cent. sodium chloride solution. A small portion of the proteid became insoluble after dialysing and drying over sulphuric acid.

The solution used for injection was one made with 15 per cent. NaCl solution, which was previously boiled and cooled.

Physiological Action of the Globulin. The Globulin of the Seeds of the Jequirity has the same Physiological Action as the Watery Extract of the Seeds and as the Proteid body "Abrin" described by Warden and Waddell.

Local Action.—A watery infusion of the seeds when placed on the conjunctiva causes inflammation of the membrane with purulent discharge. A similar effect is produced by "abrin," and precisely a similar effect by the pure globulin. This severe local action of abrus seeds we therefore ascribe to the globulin of the seeds.

In one experiment, for example, about 0·002 gram of the pure globulin was placed on the inner surface of the left eyelid of a large rabbit. In 17¾ hours the conjunctiva was reddened and slightly swollen; there was no chemosis, but there was a clear serous discharge from the eye. In twenty-four hours more there was intense

* See "The Nature of Papain, &c.," 'Journ. of Physiology,' vol. 6, p. 344.

purulent ophthalmia with subconjunctival ecchymosis; the cornea was quite clear. The purulent discharge lasted till the death of the animal, about eighty-three hours after the inoculation. The animal was apparently ill for about four hours before death. At the *post-mortem* there was severe subconjunctival hæmorrhage, with œdema round the eyeball.

In another experiment, where a very small quantity of the globulin in a 15 per cent. NaCl solution was placed on the conjunctiva, there was inflammation with purulent discharge from the eye in 16½ hours. The inflammation rapidly increased (although there were no general symptoms of poisoning), and began to subside on the sixth day after inoculation. In this case there was no subconjunctival hæmorrhage, the amount of globulin used being less than in the first experiment quoted.

From both these experiments the local action of the globulin is evident: it produces intense inflammation, œdema, and local ecchymosis, and may produce death if the dose is sufficiently large.

General Action.—The local irritant effect is also seen when a solution of the globulin is injected subcutaneously. The connective tissue becomes œdematous over a large area, there is congestion, and if the dose be a large one there are also punctiform ecchymoses. No suppuration is found, probably because the animal does not live long enough.

As regards the general effect on the body produced by abrus-poison, we have not much to add to the account given by Drs. Warden and Waddell in their pamphlet previously quoted. Abrus-globulin produces the same effect as the substance called by these observers "abrin," and as the watery infusion of the juice.

If a small quantity of globulin, 0·0022 gram (0·01 gram per kilo. of body weight), be injected under the skin of a rat weighing 218 grams, symptoms of poisoning begin to appear in about six hours. The animal then seems a little languid, and in a condition impossible to distinguish from sleepiness. It continues in this state, making no voluntary movement, irresponsive to slight external stimuli, and with half-shut eyes. It lies huddled up in its cage, the breathing becomes more rapid, and bloody motions are passed shortly before death, which occurs in about twenty-four hours after inoculation. If the animal is with young it aborts.

Post-mortem there are signs of œdema and ecchymoses at the seat of injection, and punctiform ecchymoses also beneath the peritoneum and sometimes in the lungs. The intestines are congested, sometimes greatly inflamed; the adenoid patches in the mucous membrane are swollen, and submucous ecchymoses are often seen. The blood sometimes remains fluid for a long time, and is sometimes coagulated.

Fatal Dose.—The minimal fatal dose of the globulin has not been determined by us; the smallest dose we have found kill is 0·0022 gram or a dose of 0·01 gram per kilo. of body weight.

This dose took twenty-four hours to kill; a larger dose kills more rapidly, and we have repeatedly demonstrated the fact that the rapidity and intensity of the action of abrus-globulin is proportional to the quantity used. This is seen in the following table of experiments on pigeons:—

Fatal Dose of Abrus-globulin in Pigeons.

Weight of pigeon in grams.	Dose of globulin in grams.	Dose per kilo. of body weight.	Death occurred in
310	0·0031	0·01	About 18 hours.
397	0·15	0·373	" 8½ "
216	0·2	0·925	" 5¼ "

In analysing the general symptoms produced by abrus-globulin (abrus-poison) it is seen that there are no convulsions produced and no definite paralysis, only a general weakening, ending gradually in death.

The fact that the poison produces œdema at the seat of injection, and that death is slow, may point to the idea that the cause of death is secondary to the local lesion produced by the poison, the absorption of septic material occurring. This is not so, however, and for several reasons.

In the first place the local œdema is very slight in amount if the dose be small, and yet death occurs. Suppuration, moreover, has not occurred in any of our experiments. Moreover, if the fatal effect really occurred from the absorption of septic material, we should expect a rise of temperature; the animal would be febrile. This is not so; the temperature instead of rising falls in a most remarkable manner. A few observations were made on cats by Warden and Waddell on this point. The animals used by us were pigeons, and the temperature was taken in the rectum every twenty minutes or half hour after the injection of the globulin. At the same time the number of respirations per minute was counted, so that in the chart (fig. 1) the respiration curve can be compared with the temperature curve.

It will be seen from the chart that the temperature, which was 106·8° F. before inoculation, began to fall in less than half an hour after the poison had been injected; that this fall was gradual until two hours after the inoculation, after which time there is a rapid fall until death. The fall of temperature until death was from 106·8° F. to 83° F.; *i.e.*, 23·8° F. or 12·6° C.

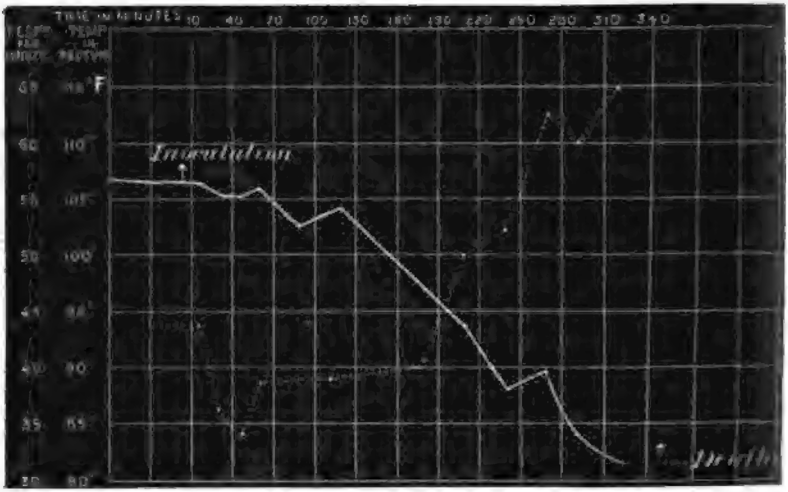


Fig. 1.—Pigeon weighing 216 grams: 0·2 gram abrus-globulin injected over pectoral muscle (0·925 gram per kilo. of body weight). Death in 5 hours 45 mins. Temperature curve, thick line; respiration curve, dotted line.

The respirations are seen from the curve to diminish more rapidly than the temperature falls for fifty-five minutes after inoculation; there is then a stationary period until 115 minutes, after which the number of respirations per minute rapidly rises until death. The curves, therefore, show a fall of temperature with an increase in the number of respirations.

In other experiments, where the temperature has been regularly taken, and the respirations counted, the same phenomena have been observed.

In an experiment where a pigeon weighing 397 grams was inoculated with 0·15 gram of abrus-globulin (0·373 gram per kilo. of body weight), the fall of temperature was at first very rapid; from 107·1° F. to 105° F. in less than half an hour, after which the temperature remained stationary (with slight fluctuations) until near death.

All the experiments point to the fact that abrus-globulin produces, when subcutaneously inoculated, a remarkable fall of temperature in pigeons; at the same time producing in them great rapidity of respiration, a phenomenon also observed in mammals.

Effect on Blood Pressure.—The effect of abrus-globulin on the blood-pressure was tested in dogs. Thus, in one experiment, where a dose of 0·01 gram per kilo. of body weight was injected into the femoral vein, there was no effect on the blood pressure in the carotid artery nor on the inhibitory power of the vagus, which was tested 15 minutes,

25 minutes, and 37 minutes after the inoculation. In only two cases have we noticed any effect; in both these there was a very slight and gradual fall in the blood pressure. In one of these cases a dose of 0·044 gram per kilo. of body weight was injected into the femoral vein, and in the other a dose of only 0·0042 gram per kilo.

There is, therefore, no marked effect on the blood pressure, nor on the inhibitory power of the vagus, such as we noticed on the body temperature. The only immediate evident effect of abrus-poisoning is a fall of the temperature; symptoms of weakness and of rapid respiration only appear much later, from five to twelve hours after the subcutaneous inoculation, according to the dose given.

Effect of Heat on the Physiological Action of Abrus-globulin.—Boiling destroys the activity of a watery extract of abrus seed. We have performed many experiments with the view of determining at exactly what temperature the activity of the poison is destroyed. It is reasonable to suppose that if the chief poisonous principle of abrus seed is the globulin, that the activity would be destroyed at the temperature at which the globulin coagulates; and this we have found to be the case.

The Poisonous Action of Abrus-globulin is completely destroyed by momentarily heating a Solution to a Temperature of 75° *or* 80° *C., the Temperature at which the Globulin coagulates.*

The heating was done in a test-tube placed in a water-bath; as soon as the solution of globulin was at the required temperature, the tube was removed and rapidly cooled.

In one experiment, four rats were inoculated with the same dose of globulin, which in one case had been heated to 50° C., in another to 60° C., in the third to 70° C., whilst the fourth was unheated; all the animals died with symptoms of abrus-poisoning within thirty hours of the inoculation. In another experiment, three rats were inoculated, the first with a solution of globulin heated to 75° C., the second with the same amount heated to 80° C., whilst the third was inoculated with unheated globulin. The first two rats, those inoculated with solutions heated to 75° and 80° C., remained quite well, while the third, inoculated with unheated globulin, died within twenty-four hours of abrus-poisoning.

Experiments performed on the eye with solutions of globulin, similarly treated, gave the same results; if heated up to 50°, 60°, or 70° C., the globulin produces as intense an ophthalmia as when unheated, but if heated to 75° or 80°, the globulin produces no inflammation or œdema.

The *conclusions*, therefore, at which we have arrived from our experiments are—

1. That the poisonous principle of the seeds of *Abrus precatorius* (jequirity) is a globulin.*

2. That the activity of this globulin is destroyed by heating its solution to 75° or 80° C., the temperature at which it coagulates.

3. That this globulin produces a remarkable fall of body temperature after subcutaneous injection.

4. That it causes rapidity of breathing shortly before death.

5. That the other actions ascribable to the globulin are: the production of local œdema and inflammation when subcutaneously injected or applied to the eye, the presence, *post-mortem*, of petechiæ beneath the serous membrane, and the occurrence of hæmorrhagic gastro-enteritis.

III. "The Toxic Action of the Albumose from the Seeds of *Abrus precatorius*." By SIDNEY MARTIN, M.D. Lond., British Medical Association Research Scholar, Assistant Physician to the Victoria Park Chest Hospital. (From the Physiological Laboratory, University College, London.) Communicated by E. A. SCHÄFER, F.R.S. Received May 8, 1889.

An account, by Dr. Wolfenden and myself, of the physiological action of the globulin which I extracted from the seeds of the jequirity plant, has been presented the Royal Society. I have shown† that there are two proteids present in the seeds; a globulin and an albumose. The present paper deals with the physiological action of the albumose.

Dr. Wolfenden and I showed in the paper referred to that the globulin possessed the poisonous qualities of the watery extract of the seeds and of the body called "abrin," described by Drs. Warden and Waddell. After being obtained in the pure state, it produced severe conjunctivitis when applied to the eye, and when subcutaneously injected it caused local œdema and ecchymosis, followed by death with the signs and symptoms of gastro-intestinal irritation and inflammation. It moreover lowered the body-temperature of the pigeon in a remarkable manner. From the method used by Drs. Warden and Waddell in preparing their "abrin," both proteids would be obtained, since they used a watery extract and precipitated the proteids with alcohol. Abrin would, therefore, be a mixture of globulin and albumose. As Dr. Wolfenden and I had found that the

* An account of the physiological action of the albumose of abrus-seeds has been presented to the Royal Society by one of us (M.).—May 10, 1889.

† 'Roy. Soc. Proc.,' vol. 42, p. 331.

globulin is a powerful toxic agent, it was desirable to ascertain whether the albumose possessed the same power and produced the same symptoms.

It is very difficult to obtain the albumose in a pure state separate from the globulin. Boiling the solution, of course, readily precipitates the globulin, leaving the albumose in solution, but as heat destroys the activity of abrus-poison, it cannot be employed in separating the two proteids. Both proteids are also thrown down by saturating their solution with neutral ammonium sulphate. The precipitate thus formed can be redissolved and the solution dialysed, thus removing most of the salt and precipitating the globulin. But I found many objections to this method. The dialysis has to be prolonged over a week, and there is thus great liability to decomposition. Moreover, it is practically impossible to precipitate all the globulin by dialysis, and the ammonium sulphate, traces of which still remain, being itself poisonous, would be likely to vitiate the result in testing the toxic action of the proteid. I therefore abandoned this method and tried the following, which answered perfectly. A concentrated watery extract of the seed was made and filtered direct into an excess of absolute alcohol. The copious precipitate which fell consisted of globulin and albumose. After a few days, the proteids were removed by filtration, washed with alcohol, redissolved in water, and reprecipitated by absolute alcohol. They were allowed to remain under absolute alcohol for several months in order to coagulate the globulin, and were then filtered off, redissolved, and reprecipitated by alcohol, and allowed to remain under alcohol for a few months longer. Altogether some of the proteids were allowed to remain under alcohol for eight months, or longer. At the end of this time they were removed, washed with alcohol, and dried over sulphuric acid. The residue was ground into a yellowish-brown powder, and consisted of coagulated globulin and of unaltered albumose.

For the purpose of inoculation this powder was mixed in distilled water, which had been well boiled to sterilise it and then cooled. The mixture was filtered and the filtrate was clear. It gave the following reactions :—

1. Neutral to test-paper.

2. No precipitate on boiling.

3. Acetic acid gave a precipitate, which mostly redissolved on boiling, coming down again on cooling, and so on. After boiling and cooling, the precipitate was readily soluble in dilute potash, showing that the proteid was not coagulated.

4. Nitric acid caused a precipitate, mostly soluble on heating, coming down again on cooling, &c. This precipitate, like the acetic acid one, is also readily soluble, after being heated, in dilute potash.

5. Copper sulphate gave a precipitate, soluble in excess of the reagent.

6. Copper sulphate and potash gave a "biuret" reaction.

7. Mercuric chloride gave a precipitate, insoluble in excess of the reagent.

These reactions are similar to those already described by me in the paper quoted ('Roy. Soc. Proc.,' vol. 42) with the exception of the behaviour of nitric acid. I stated in my previous paper that nitric acid gave a precipitate in a solution of the albumose, only in the presence of sodium chloride. This still holds true for *dilute* solutions of the albumose; in *strong* solutions, nitric acid gives a precipitate, even if the neutral salt be absent or present in very small quantities.

Fatal Dose of Albumose.—In my earlier experiments I simply weighed the quantity of dry powder to be injected; but this is not so accurate a method as the one I adopted later. About 0·5 gram of the powder was dissolved in sterilised normal saline solution (0·75 per cent.) and filtered. The amount of proteid dissolved in the filtrate was estimated by dropping a measured quantity of the liquid (1 c.c.) into about 30 c.c. of absolute alcohol, which precipitates both the proteid and the small amount of salt in solution. The precipitate and liquid were well boiled together, the precipitate removed, dried at 110° C., and weighed. The weight, deducting the amount of salt, equals the quantity of proteid present in solution. This I found the only really accurate method of estimating the dose of proteid used in inoculation.

In one experiment, 1·3 milligram of albumose was injected under the skin of a rat weighing 197 grams, being a dose of about 6·6 milligrams per kilo. of body weight. The animal was very ill 49½ hours after inoculation, but completely recovered. Double the above dose, viz., 2·6 milligrams, was injected under the skin of a rat weighing 134 grams (19·4 milligrams per kilo. of body weight), but no poisonous symptoms were noticed. A fatal result is, however, noticed if the dose be as large as 60 milligrams per kilo. of body weight; thus a dose of 10 milligrams killed a rat weighing 167 grams within 20 hours.

Symptoms.—The symptoms produced by the albumose closely resemble those noticed when the globulin is hypodermically injected. There is gradually increasing weakness, with rapid breathing, without the occurrence of convulsions or any paralysis.

On the temperature of pigeons the albumose has the same effect as the globulin. In one experiment death was caused in a pigeon by the albumose in 11 hours and 20 minutes after inoculation. The temperature, which at the time of inoculation was 107·6° F., fell in 4 hours and 5 minutes 4·6°, *i.e.*, to 103° F., after which it began to rise.

In another experiment, a pigeon weighing 335 grams was given hypodermically a dose of 20 milligrams albumose. equal to 60 milligrams per kilo. of body weight. In $4\frac{1}{2}$ hours, the animal began to show symptoms of poisoning, and died in about 6 hours or rather longer. As shown in the accompanying chart the temperature began to fall from the first, and with a few rises continued to fall until the animal was nearly dead, when the observations were ceased. The curve of the number of respirations per minute follows very closely the temperature curve, until just before symptoms of poisoning appeared, when, as will be seen in the chart, the respiration curve does not follow the temperature curve.

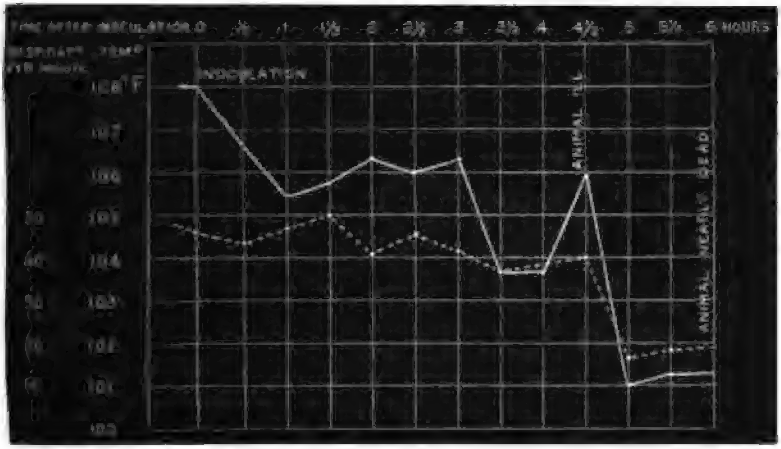


Figure showing effect of Abrus-albumose on temperature and respirations of the pigeon. Temperature taken in rectum every half-hour. Dotted line, respiration; thick line, temperature.

Post-mortem Signs.—These are the same as noticed after death from the globulin. There is local œdema and sometimes ecchymosis, with internally gastro-enteritis, and occasionally petechiæ in the serous membranes.

The blood is in most cases dark and fluid for a long time after death. It may be semi-coagulated.

Effect on the Eye.—The albumose causes severe conjunctivitis when applied to the eye. Thus 1 milligram of albumose dissolved in 2 minims of previously boiled distilled water, instilled into the eye, causes severe conjunctivitis with chemosis in less than 24 hours, and leaves at the end of six days a steamy cornea with leucomata; there are also sub-conjunctival ecchymoses.

Effect of Heat on the Activity of the Albumose.—The poisonous

action of the watery extract of abrus-seed, containing both globulin and albumose, is completely destroyed by boiling the solution. Wolfenden and I have shown in our previous paper that if the solution of the globulin be momentarily raised to 80° C., or between 75° and 80° C., its activity is once and for ever destroyed.

I have tested the behaviour of the albumose in a similar way.

If heated to 50° C. the albumose still retains its power of producing severe conjunctivitis, and of causing death when subcutaneously injected.

If heated to 70°, or 75° C., the albumose is still poisonous, but not to nearly so great a degree as if unheated or heated only to 50° C. If heated to 80° C., this diminishing effect of heat on the activity of the poison is still more seen; so that a solution of albumose containing a lethal dose, and so treated, does not produce poisonous symptoms so soon as an unheated solution, and the animal may recover.

These points are brought out in the following experiments, selected out of many similar ones, all of which were confirmative.

Experiment II. October 13th, 1888.—An equal and lethal dose of albumose injected under the skin of each of three rats. A is unheated; B heated to 75° C.; and C heated to 80° C., previous to inoculation.

A.	B.	C.
White rat, weight 162 grams. Unheated albumose.	Rat, weight 153 grams. Albumose heated to 75° C.	Rat, weight 108 grams. Albumose heated to 80° C.
11 A.M., Oct. 13, 1888. Inoculation made under skin of back.		
2 P.M. No change in any of the animals.		
11 P.M. Animal languid and drowsy. Not easily roused by stimulation. Huddled together. Quite quiet, and not breathing very rapidly.	No change.	No change.
Oct. 14, 1888. 11 A.M. Found dead. *P.M.*—Rigor mortis well marked. Body cold. Slight œdema at site of injection. No other morbid appearances.	Found dead. *P.M.*—Rigor mortis well marked. At site of injection, subcutaneous œdema. Small amount of sticky fluid in peritoneum. No further morbid appearances.	Huddled together. Quite quiet. Breathing rapid. Animal dying. Died during the day.

The above experiment shows the delay of poisonous symptoms produced by heating the albumose up to 75° C. and to 80° C.

In the following experiment, the same fact is brought out, but the result shows recovery from a fatal dose of albumose, heated up to 80° C., and no poisonous symptoms after heating the albumose to 85° C.

Experiment III. November 1st, 1888.—An equal and lethal dose of albumose injected under the skin of three rats:—A is unheated; B is heated to 80° C.; and C to 85° C.:—

A.	B.	C.
White rat, weight 142 grams.	Rat, weight 126 grams.	Rat, weight 125 grams.
Nov. 1, 1888.		
11.20 A.M. Inoculated.	11.10 A.M. Inoculated.	11.15 A.M. Inoculated.
3.30 P.M. No change.	No change.	No change.
Nov. 2, 9.30 A.M. Animal died. Rigor mortis came on immediately.	Animal ill. Quiet, huddled together, and breathing very rapidly.	No change.
	At 12 symptoms became aggravated.	No change.
Nov. 3, 10 A.M.	Animal quite recovered. Breathing normal. Takes its food well.	No change. Animal remained quite well.

Similar results were obtained in testing the effect of the albumose in producing conjunctivitis. Raising the solution to a temperature of 75° C. and 80° C. diminishes the effect but does not prevent conjunctivitis developing; the conjunctivitis is less with albumose heated to 80° than with that heated to 75°.

Experiment VI. April 17th, 1889.—One milligram albumose dissolved in 2 minims of distilled water dropped into right and left eye of rabbit. That dropped into left eye was previously heated to 80° C.

Right Eye.	*Left Eye.*
Unheated albumose.	Albumose heated to 80° C.
April 17, 1889, 10.37 A.M. Inoculation.	10.32 A.M. Inoculation.
„ 18, 10 A.M. Severe conjunctivitis with chemosis.	10 A.M. Very slight inflammation and a little purulent discharge.
„ 23, 10 A.M. Cornea cloudy; with one leucoma. Subconjunctival ecchymosis.	10 A.M. Quite normal.

The conclusions from these experiments may thus be summed up:—

1. *The poisonous activity of abrus-albumose is weakened by momentarily heating its solution to a temperature of* 70°, 75°, *and* 80° *C.; and the higher the temperature the greater the diminution.*

2. *The activity of the albumose is completely destroyed by heating its solution up to* 85° *C.*

This is about five degrees higher than the temperature at which the activity of abrus-globulin is destroyed.

Remarks on the Results obtained.—It is impossible not to be struck with the resemblances in chemical composition between abrus-poison

and the toxic principle of snake-venom. Weir Mitchell and Reichert* have shown that in the American rattlesnakes, the venom contains two poisonous proteids, globulin and a "peptone." The coagulation temperature of the globulin described by them is between 60° and 70° C. The peptone is not a true peptone, as physiological chemists now understand the term; since it is, according to Mitchell and Reichert, precipitated by acetic acid and potassium ferrocyanide. Their peptone, indeed, seems more allied to the albumoses. In the venom of the mocassin, *e.g.*, the "peptone" is precipitated by adding an excess of NaCl to the solution, besides being thrown down by dilute acetic acid. The peptone found by these observers in cobra-venom is precipitated by acetic acid and potassium ferrocyanide, as well as by NaCl added to saturation. A true peptone is not precipitated in this manner, and I cannot but conclude that the body found by Mitchell and Reichert is closely allied to the albumose class of proteids.

The globulin of abrus-seed coagulates between 75° and 80° C. in 10 per cent. magnesium sulphate solution and between 66° and 73° C. in 10 per cent. NaCl solution.† The coagulation temperature in the last solution therefore nearly corresponds to the coagulation temperature of the venom globulin of the rattlesnakes. Mitchell and Reichert do not mention in the presence of which salt the globulin was coagulated. Sodium chloride distinctly lowers the temperature of coagulation.

Abrus-albumose, moreover, closely resembles the "peptones" and "peptone-like" bodies found by the observers in snake-venom. Like them it is uncoagulated by heat,‡ it is precipitated by acetic acid, and by acetic acid and potassium ferrocyanide, and also by saturation with sodium chloride in an acid solution.

Other observers have described in cobra-venom a poisonous albumin and acid albumin (Wolfenden).

Nature of Poison.—I do not propose here to discuss fully the nature of snake-venom. The results of Mitchell and Reichert's researches I would wish to refer to are:—(1) That both the globulin and peptone-like body present in the venom of the rattlesnake are poisonous; (2) that the severe local ecchymosis produced by viperine venom is due to the globulin present in it, since such local effect does not occur after inoculation if the globulin be removed from venom. The third point I would wish to refer to is the effect of heat on the activity of rattlesnake venom. If the venom of the Diamond head (*Crotalus adamanteus*) be heated up to 74° C. the venom is still active; if up to

* 'Researches upon the Venom of Poisonous Serpents.' Philadelphia 1885.

† 'Roy. Soc. Proc.,' vol. 42, p. 333.

‡ Mitchell and Reichert state that cobra-venom may be boiled and filtered; and the filtrate will after a time give a further precipitate on boiling. They explain this by saying a coagulable body is formed from a non-coagulable.

76·5° C. a few symptoms follow a lethal dose, but recovery takes place; while if heated up to 79·5° C., 81°, and 100°, a lethal dose of the venom does not produce death. The effect of heat varies with different snake-venoms. It takes prolonged boiling to destroy the activity of cobra-venom; and in some of the American snakes simple boiling does not completely destroy the activity of the venom, although it diminishes it. These results are explicable in the consideration that by coagulation the activity of the globulin is destroyed, and by prolonged boiling the peptone or peptone-like body is decomposed.

Abrus-poison, both globulin and albumose, produce, like snake-venom, a local lesion, viz., inflammation and œdema, with ecchymosis; but the activity of venom in producing this result is enormously greater than that of abrus. It is not the globulin alone of abrus that produces the local lesion, but also the albumose. As in many cases of such poisoning, also, the blood after death is in a fluid or semi-fluid state.

The effect of heat on abrus-poison is more marked and definite than on snake-venom. The physiological activity of the globulin is, *e.g.*, completely destroyed at about its coagulation temperature, 80°C., while the activity of the albumose is not destroyed until the solution is raised to 85° C.

Nature of Abrus-poison.—To explain the action and nature of abrus-poison, two theories may be stated:—

1. That the poison is of ferment-nature attached to the proteids.
2. That the proteids develop by contact with living tissue a body or bodies which are poisonous.

The first idea is only supported by the fact that the activity of both poisonous proteids is destroyed at about the temperature at which digestion ferments are destroyed. At the same time, there is no evidence to show that such a temperature does not so alter the constitution of the molecule of the proteids that they do not produce by contact with living tissue toxic principles. Since there is no accurate knowledge of the constitution of the proteid molecule, the question as to why one proteid should be poisonous and another harmless must remain unsettled. Although this is so, the results obtained in the experiments on the abrus-poison are definite, and may be thus summarised:—

1. The poisonous activity of the seeds of *Abrus precatorius*, the jequirity, resides in the two proteids present in the seeds—a para-globulin and an albumose.
2. Both of these proteids have practically the same action. They produce severe conjunctivitis when applied to the eye; and when subcutaneously injected they cause local inflammation, œdema, and ecchymosis, and gastro-intestinal irritation, with extrusion of fæces

and blood; the general symptoms being, first, a great fall of the body temperature, and a condition of stupor, ending in death.

3. The activity of both these proteids is destroyed by moist heat. In solution the activity of the globulin is destroyed at between 75° and 80° C., and that of the albumose between 80° and 85° C.

4. That abrus-poison resembles snake-venom in chemical composition, in the local lesions produced, in producing a fall of body temperature, in causing semi-fluidity or fluidity of the blood after death, and, to some extent, in the effect of moist heat on it. Abrus-poison is, however, much less active than snake-venom.

The following table shows a comparison between the activity of the venom of various snakes and of Abrus:—

Vipera berus (common adder)..	Fatal dose in man 0·0021 gram per kilo. of body weight (Fontana).*
Hoplocephalus curtus (Australian tiger snake)† ..	Fatal dose in dog, 0·00485 gram per kilo. of body weight; ¼ a grain in medium size dog (15 lbs.).
Cobra	Fatal dose in dog, 0·000079 gram per kilo. of body weight; $\frac{1}{10}$ grain in dog weighing 18 lbs. (Vincent Richards).‡
Abrus-poison:	
Globulin................	Fatal dose, 0·01 gram per kilo. of body weight.
Albumose...............	Fatal dose, 0·06 gram per kilo. of body weight.
Peptic albumoses	Fatal dose in dog, any dose over 0·3 gram per kilo. of body weight (Pollitzer).§

IV. "On the Early Development of *Lepidosteus osseus*.—Preliminary Notice." By J. BEARD, Ph.D., B.Sc., Zoologist to the Scottish Fishery Board, Edinburgh. Communicated by Professor T. H. HUXLEY, F.R.S. Received April 20, 1889.

In the spring of 1888 I journeyed to North America for the purpose of collecting material for a study of Ganoid development.

I sought and found even more material than I wanted in the now well-known habitat of *Lepidosteus*, Black Lake, N.Y. No better hunting-ground could be wished for by the morphologist in search of Ganoid material. The lake contains *Amia*, multitudes of *Lepidosteus*, and (it is said) sturgeons. One need not be at much trouble in seeking sturgeons, for the River St. Lawrence, which flows within 12 miles of Black Lake, will vie with any Russian river. I made the

* Quoted in Marx, 'Gift-Lehre,' vol. 2, p. 74.

† "Report of Special Commission on Snake-poisoning." 'Australian Med. Journal,' 1876, No. 21, p. 104.

‡ 'Landmarks of Snake-poison Literature.'

§ 'Journal of Physiology,' 1886.

acquaintance of fishermen on the St. Lawrence who assured me, and I often verified the fact with my own eyes, that each one of them catches from 60 to 70 sturgeons per week. Many attempts were made to fertilise sturgeon eggs, but all were unsuccessful, probably the season was too early. *Lepidosteus* material was fairly easily obtained, and a large collection of embryos and many adult fishes were secured.

I owe my thanks to the Government Grant Committee of the Royal Society, for the substantial grant which enabled me to defray the cost of collection, &c., of the material used in the following investigations.

It is proposed to give an outline of the development of *Lepidosteus* during the first three weeks of its life; by the end of this time pretty nearly all the organs are developed, and the larva has acquired many of the adult characteristics. The early development, *i.e.*, that of the first four days, is very difficult of investigation, far more so than that of Amphibia and Marsipobranchii. The reason of this lies with the yolk, for that outside the embryo gives rise to technical difficulties, while that which fills all the cells renders everything blurred and indistinct. Fortunately, much can be made out by means of surface views, and these help very materially in rendering an interpretation of the sections possible.

The Egg.—I have little to add to Balfour and Parker's account. As they state the ripe *eggs* are *spherical bodies* of 3 mm. in diameter. The *egg-shell* is composed of two parts: (1) externally a layer of pyriform bodies, and within it (2) a zona radiata. No micropyle could be detected.

The *pyriform bodies* are certainly modified cells, each with the remains of a nucleus at its outer end. These modified cells have degenerated into a sort of glue, which causes the excessive stickiness of the newly laid eggs.* Such a layer, though less developed than here, is characteristic of the eggs of sturgeons and *Petromyzon*.

In the ovarian egg these "pyriform bodies" are probably nutritive cells to the ovum, for their outer ends near the nuclei contain a number of minute yolk particles.

The inner egg membrane is not composed of two layers either in *Lepidosteus* or in the sturgeon. It is a simple zona radiata, the striæ reaching to the innermost portions of the membrane. The division into two layers, sometimes seen, is the optical effect of thick sections.

Within lies the egg proper. It consists of an outer protoplasmic layer containing small yolk particles, and a central yolk mass, *free from protoplasm*, and made up of much larger yolk plates. There is a large germinal vesicle with membrane, the vesicle containing a rather large number of chromatin bodies.

* In alkalies, baryta, and lime-water the egg-shell swells up, and the two latter reagents are valuable allies in getting rid of the egg membranes in young stages.

When the egg is ripe, the germinal vesicle lies just under the shell in a protoplasmic mass, which is permeated by very small yolk particles. The sturgeon egg has a very similar structure, but the yolk elements are much finer. The males are on the average only half the size of the females, and are very much more numerous than the latter, as others have noted. The spawning takes place between May 20th and the middle of June. Some of the inhabitants of the shores of Black Lake have seen it taking place in July. This appears to me very likely. The fishes only spawn on warm windless days, and if cold weather comes, they may remain away from the "points" for weeks, as Garman already noted.

I observed the spawning on three occasions, on May 24th, June 8th and 9th. In the interval no eggs were laid, though search was made for eggs and fishes every day by my assistants or myself. The spawning takes place during the heat of the day, between 12 and 3 o'clock. A full account of it has been already given by Agassiz, and quoted by Balfour and Parker. The eggs are thickly sown, and cling to the small stones which lie about in the shallow water of the "points."

Certain spots seem to be traditional egg-laying grounds for *Lepidosteus*. The eggs are pretty easily removed from the stones, and my own were hatched out in a Californian salmon hatching apparatus.

Most of the eggs left on the stones fell a prey to fungi, and very few indeed hatched out. With care and constant attention a much greater percentage can be hatched out in the apparatus. The difficulties of the investigation prevented the observation of the formation of polar bodies. The segmentation is very rapid, and a cap of small cells is formed in the course of five or six hours. The segmentation is very unequal *but in a sense complete.* Eight furrows can be traced to the centre of the lower pole. The attempt to segment the lower hemisphere is, however, soon given up, none of the eight furrows penetrate very deeply into the yolk, and none reach the centre by a long way. They are only superficial furrows.

The cell cap of the upper pole increases rapidly by divisions within itself, and by the addition of fresh segments from the incomplete segments below its margin. It grows over the yolk, and in the course of 24 hours or less completely encloses the latter. A beautiful circular blastopore is formed, from which a yolk-plug projects as in Amphibia.

Before the close of the blastopore, the region of the future embryo is marked out by a blastodermic thickening, extending forwards from the edge of the blastopore.

A groove marking the axis of the embryo soon appears in this thickening. It must be remarked that this groove appears only just before the closure of the blastopore, and hence it cannot in *Lepidosteus* be regarded as part of the latter.

The blastopore closes on the second day, and *at no time is a canalis neurentericus formed.*

The embryo becomes more and more distinctly marked out, and the nervous system is differentiated.

By-and-bye the embryo gets raised above the level of the blastoderm, and a solid tail-bud is very early rounded off above what was the anterior margin of the blastopore.

The *mesoderm* or *mesoblast* arises very early, and before the close of the blastopore.

If embryologists have not yet agreed as to its mode of origin in the chick, how can one hope to settle this question in the difficult material of *Lepidosteus*? It appears to me to arise from the epiblast on each side of the middle line, and from the epiblastic region at the lip of the blastopore.

I make these statements with the utmost diffidence. Apart from its medium thickening, *the epiblast* is very early divided into two layers. The outer or covering layer (*Deckschicht* of the German authors) takes no share in organ formation at all. It covers the embryo everywhere, but no organs are formed from it. It may, perhaps, be compared to the skin of a larval Annelid. The inner layer may be spoken of as the *formative epiblast.*

The hypoblast has only a dorsal or neural extension, the ventral side of the embryo being occupied by yolk.

Epiblastic Organs. Nervous System.—This is formed solely out of the formative epiblast, though it must be noted that the outer layer is often grooved in the middle line. It may be regarded as a folded plate of epiblast, which sinks below the rest of the formative epiblast, and it differs solely in its mode of formation from the nervous systems of Elasmobranchii and *Amphioxus* in that the folds are closely applied to each other, and only separate later on in development. The brain vesicles can soon be distinguished, and in later stages a similar apparent segmentation can be traced for some distance along the spinal cord. The nervous system becomes hollowed out by the separation of its walls from each other. The optic vesicles arise as a pair of hollow evaginations of the fore-brain. The lens also is a product of the formative epiblast.

In the central nervous system two structures are very early distinguishable:—

(1.) The transient *giant ganglion cells* in the spinal cord; these are described in a separate section (p. 117).

(2.) The *ciliated groove* which I have elsewhere described as forming the floor of the primitive central canal. It persists in the adult, and there also forms only the floor of the central canal as in sharks and elsewhere.

The Brain.—The fore-brain roof in embryo and adult is very thin,

non-nervous, and epithelial in character, as in Marsipobranchs. Teleostei, and other Ganoids (*Acipenser*, *Polypterus*, and *Amia*).

The nervous elements of the fore-brain are represented by thick basal and lateral structures of a solid nature, forming the so-called "corpora striata." To what extent they are homologous with corpora striata shall be discussed in my full paper. To my mind there appears to be no evidence to show that the fore-brain region of Marsipobranchii, Teleostei, or Ganoidei is a degenerate structure. The pineal gland and the nervous part of the pituitary body or hypophysis cerebri have the usual mode of formation. In the adult, the pineal body arises by a hollow stalk from the roof of the thalamencephalon, and it proceeds forwards over the epithelial roof of the fore-brain, ending in a large simple flattened vesicle. This vesicle has a striking resemblance in structure to the same organ in *Myxine*. It lies in a mass of adenoid tissue, which, however, is no part of the pineal body. Waldschmidt's statements under this head in *Polypterus* will require modification.

The first development of the oral part of the hypophysis cerebri is difficult of investigation, on account of the distortion produced by the developing suckers. It arises as a medium solid ingrowth of the formative epiblast, slightly in front of, or almost within, the mouth involution. Its direction is towards the infundibulum and the end of the notochord. It does not become hollowed out as in many other forms.

In the adult the hypophysis cerebri possesses a more complicated structure than the same organ in *Polypterus*, containing as it does glandular epithelium, lymphoid tissue, and degenerated nerve-cells. Its duct does not persist as in *Polypterus*.

Regarding the spinal cord, only one further remark need be made, that is, that behind the anus it is for some time solid.

The Nerves, Ganglia, and Peripheral Sense Organs.

As Balfour and Parker state, the nose is formed as an invagination of a certain portion of the formative epiblast. This is not remarkable when we remember that no organs are formed from the outermost layer.

The ear arises in the same way. The otoliths of *Lepidosteus* and *Salmo* are formed similarly to the otoliths of Invertebrates. Certain cells of the lining of the auditory vesicle become freed from the epithelium, and lie loosely in the auditory cavity. They acquire a position in rows just over the sensory hairs. The cells become calcified, and their nuclei disappear.

The otoliths are probably formed by the fusion of a number of these calcified bodies.

The lateral or branchial sense organs are also products of the formative epiblast.

The cranial and spinal ganglia are formed as recently described by me in sharks and birds.

In the mouth involution also the formative epiblast is alone concerned, the covering layer passes over from the anterior end of the head to the yolk sac. The mouth breaks through about the fourth day. The anus arises as a solid ingrowth of the formative epiblast in the place where the blastopore once existed.

The Larval Suckers.—These organs also are products of the inner or formative epiblast. They are developed very early, and commence to be differentiated about the third day of egg development. They are formed as a number of closed spherical sacs, one part of the wall being thin, and this part is ruptured when they break through the skin a day or two before hatching. One of these sacs is median, and arises almost within the mouth involution. It could almost be mistaken for the developing hypophysis, were it not for its subsequent fate and for the fact that it is directed forwards. When it breaks through the skin it forms a flat glandular plate on the roof of the mouth. From its position it can hardly be of much use as a sucker.

The functional suckers are composed of two sorts of cells: (1) Long glandular cells with hyaline slightly granular contents and nucleus lying near the inner end of the cell. Between these (2) supporting cells with nucleus in the middle of the cell.

Each glandular sucker has the structure of and is probably serially homologous with the suckers of larval Anurous Amphibia. During the period at which they lie beneath the covering epiblast, and for a short time after, the arrangement of the suckers is distinctly bilateral. The median one appears to be unpaired at all stages. They form a complete circlet on the disk, with additional suckers within the circle. On the whole, the organ may be said to be composed of two fused circlets of suckers. Balfour and Parker noticed the numerous vascular channels in the mesoblast just inside the suckers.

The young *Lepidosteus* I possessed did not make use of the suckers for several days after hatching.* The suckers commence to degenerate when the young fishes are about three weeks old.

The pad at the end of the adult snout occupies the position of, but cannot be considered as arising from, the larval suckers. The latter are transient organs.

Hypoblastic Organs.—The origin of the notochord is not yet clear to me, for it arises during an epoch in which the processes of development are difficult of interpretation on account of the yolk.

There is at first a solid œsophagus, as Balfour and Parker state, and the posterior gill clefts arise as solid evaginations from it. The

* The first batch of eggs hatched on the ninth day, the second on the seventh.

spiracular cleft or rather its rudiment is a very early formation, being developed long before hatching. The evagination to form it reaches the formative epiblast and fuses with it, this fusion persisting for a long time, but at no time can a lumen opening on the exterior be detected. As Wright has shown, a part of the spiracular cleft persists in the adult in the form of a canal leading upwards and forwards into the region of the periotic capsule.

The first branchial cleft is formed long before the others and before hatching. In newly hatched larvæ it has a wide opening on to the exterior. This opening soon becomes apparently less by the growth of the operculum.

The pneumatocœle arises at a very early period and long before hatching. It is a fold of the neural median hypoblast, and grows backwards in length apart from connexion with the alimentary canal. For some time the ventral part of the alimentary canal is only filled in by yolk, by-and-bye the gut becomes shut in by ingrowths from the sides in the manner suspected by Balfour and Parker.

The *somites*, like the mesoblast from which they are formed, are at first solid: they are long and narrow. As is usual, their mode of formation is from before backwards; but in front of the first one formed, two others appear later on just behind the auditory capsule. These latter are, as their fate shows, the two anterior somites of the hypoglossus. Regarding head-somites, I prefer at present to say nothing.

The inner wall of the somites gives rise to muscle, and most of the outer wall is converted into pigmented connective tissue. The somites become much elongated, and their ends are constricted off as buds to form the musculature of the paired and unpaired limbs. Each end of each somite constricts off a single bud, which only divides into two at a later stage than the twenty-first day. The posterior paired fins only begin to develop towards the end of the third week, and the muscle buds of the unpaired fins remain in an embryonic condition beyond this period.

Urinogenital System.—To form the *pronephros* there is a solid evagination of the mesoblast, uniting the somites with the somatopleure and splanchnopleure. This arises early on the third day, and reaches from the 4th to 8th or 9th somites inclusive. This probably fuses with the epiblast, and at any rate a solid segmental duct is formed—probably from the inner epiblast layer. This grows gradually backwards, having at first an indistinct form of termination, finally it reaches the proctodæum, and becomes fused with it.

After this stage, two additional mesoblastic somites are formed behind the auditory vesicle. Then the first three somites behind the ear are those of the hypoglossus, and, as van Wijhe has determined

in Elasmobranchii, the pronephros begins at the third definite body-somite, or, counting the three hypoglossus somites, at the sixth. Its extension backwards varies, but the region of three somites is always concerned in its formation. It may extend at first over five or six somites, but the part beyond the anterior three soon aborts. In some cases it has a greater extension on one side of the body than on the other.

Three funnels seem, as a rule, to be formed on each side of the body, but the most posterior of these disappear, two pairs being left. These persist throughout the larval period. The ciliated openings into the body-cavity become narrowed. By this narrowing of the opening, and the widening of the part opposite to the glomerulus, the pronephric chamber described by Balfour and Parker arises. It need scarcely be remarked that there are two of these "chambers" on each side. Even when the mesonephros is in course of development, *i.e.*, from the 16th to the 18th day, the two funnels on each side still persist and are quite distinct.

As just stated, the *mesonephros* arises between the 16th and 18th day. The date of its development seems to be variable. Its tubes are formed in the angle of the body-cavity between the region of the segmental duct and the genital ridge; the latter is at this period part of the mesentery of the alimentary canal. One notices that the cells of this region of the cœlom are filled with yolk. These yolk-filled cells give rise to the mesonephros and the genital glands. The storing-up of yolk reminds one of Dohrn's discovery of yolk-laden cells in the head-somites of *Ammocœtes*, *i.e.*, in those structures which at a much later period form the eye-muscles of the *Petromyzon*. At the 16th day the somites have been almost entirely converted into muscle and connective tissue, and in *Lepidosteus* the mesonephros can neither be derived from the part connecting the somites and body-cavity together, as in Elasmobranchs and birds (Sedgwick; van Wijhe), nor from part of the somites (nephrotome of Rückert). There is here no intermediate cell-mass as in the chick, but the mesonephric tubules arise as distinct segmental evaginations of the wall of the body-cavity at the point indicated above. They grow over the segmental duct in a curved fashion, and open into it by piercing through its wall.

Such is the brief account of the early development of *Lepidosteus* which I feel at present in a position to give, and I close this communication with a notice of—

A Transient or Larval Nervous Apparatus in Lepidosteus and certain other Ichthyopsida.

In the course of my investigations I noticed the very frequent occurrence of "giant ganglion cells" in a particular situation along

the whole length of the spinal cord of *Lepidosteus* and certain other fish embryos. So little had hitherto been published about their occurrence in different groups of fishes, that a comparative investigation of the matter seemed desirable; all the more, as I soon arrived at conclusions as to their meaning and fate very different from those of Dr. Paul Mayer, the only author who has paid much attention to such cells (in the Scylliidæ). In all cases these giant ganglion cells occupy the same typical position in the extreme dorsal or neural border of the spinal cord. They are found in very young embryos in nearly every transverse section through the region of the spinal cord. Very often a pair, one on each side of the middle line, is met with in a single section. To obtain a clear insight into their distribution horizontal longitudinal sections are necessary. When the sections of such a series are examined, one notices that the first sections which pass through the dorsal or neural limit of the spinal cord, contain a large number of rather large ganglion cells, and it can easily be verified that the roof of the spinal cord along its whole length is composed of similar cells, which form a double row reaching from the termination of the hind-brain to the posterior limit of the central nervous system.

Their occurrence in the brain region proper is very doubtful; as they appear to extend forwards only as far as the anterior boundary of the hypoglossus region. That is, in early stages they can be traced as far as those somites which belong to the hypoglossus.

They are the first cells in the embryo which develop ganglionic characters, and they are fully developed in young embryos long before the remaining cells of the nervous system become ganglionic. The cells are multipolar; and in some cases processes can be seen passing from them into the developing spinal cord, but I cannot say how they are connected with other nervous elements. Possibly they are paired; at any rate, in many cases they are bilaterally arranged, and in the region of each mesoblastic somite in the different fishes to be presently mentioned from four to eight pairs of such cells occur. The exact number can only be determined by means of reconstructions of embryos. There is probably a defined number of them in every embryo of each species, and this number must be several hundred.

The most remarkable circumstance is their fate, which I have so far fully determined in *Scyllium*, *Pristiurus*, *Lepidosteus*, *Salmo*, and *Triton*.

On the Formation of the Permanent Central Canal of the Spinal Cord.

These Ganglion Cells are all shut out of the Central Nervous System. Their processes are either withdrawn or cut off, more probably the latter, and their poles now present a curious stumpy appearance. The

cells persist for a long time, lying outside the cord, and on its dorsal or neural surface, just over the posterior fissure.

Gradually they undergo a series of degenerative changes. The stumpy processes vanish; the cells shrink, and so get smaller. Finally, they become glassy, having lost all traces of nucleus and nucleolus, and disappear.

In fact, the series of changes undergone by these cells corresponds exactly to that degeneration and death of nerve-cells, which the pathologists term simple atrophy (*einfache Atrophie*, see Ziegler's 'Pathologische Anatomie,' 3te Auflage, 2ter Theil, pp. 603—606). Though the presence of giant ganglion cells in the embryos of certain Scylliidæ was known,* till now we had no idea of their distribution over the whole spinal cord as stated above. Like Dr. Mayer, I find them in *Scyllium* and *Pristiurus*, in both of which forms they are exceedingly large, numerous, and well developed. Along with Mayer I failed to detect them in *Torpedo*, but met with them in *Raja*. I also miss them in *Acanthias*, but in *Mustelus*, where as a normal thing they do not develop, one may find about a dozen of them well developed in a single embryo, but then in abnormal situations, lying free in the formative tissue of the mesoblast, and outside the central nervous system. They are very obvious in 10 millimetre embryos of *Salmo*, and may easily be detected in *Labrax*, *Esox*, and *Rhodeus* embryos of the proper age. They are very numerous in newly hatched *Lepidosteus*. Only with difficulty can they be demonstrated in young *Petromyzon* embryos, on account of the yolk filling the cells, but they are certainly present in this form. At present I do not possess sufficient material to follow their fate in *Petromyzon*, but doubtless it is the same as in the three groups represented by *Scyllium*, *Salmo*,† and *Lepidosteus*.

In *Raja*, *Labrax*, *Esox*, and *Rhodeus*, I have not followed all stages of their degeneration, but I have studied this sufficiently to be sure that their fate is that of the ganglion cells in the groups just mentioned. They may be found in larvæ of *Rana* and *Triton*, and in the latter form they have the usual fate—that I have determined. *It is very significant to notice that the forms in which they normally occur are, without exception, oviparous.*

The abnormal occurrence of a dozen or so giant ganglion cells in *Mustelus* and their presence in *Raja*, coupled with their absence in *Torpedo*, are interesting facts, which point to the conclusion that the viviparous Elasmobranchii once possessed them as a normal development.

The giant ganglion cells which occur in adult *Amphioxus* and

* Paul Mayer: "Die Unpaaren Flossen der Selachier." 'Mittheilungen Zool. Stat. Neapel,' vol. 6, pp. 228—229.

† They probably occur in nearly all Teleostei. According to Eisig they have been seen by P. Mayer in many marine forms belonging to this group.

Petromyzon appear to have no homology with these larval ganglion cells.* The proof of this statement is impossible without figures. I hope to show, in a fuller paper on the early development of the central nervous system, that in *Scyllium* giant ganglion cells are developed in deeper portions of the spinal cord, and that these cells have exactly the situation and characters of the well-known giant ganglion cells of *Amphioxus*.†

In the same sections of *Scyllium* embryos the two sorts of cells can be seen; the one deeply situated in the cord, and with well developed processes, the other outside the nervous system, and greatly degenerated.

I will here only remark that I cannot support Mayer's conclusions‡ as to the fate of these giant ganglion cells, and defer a discussion of his views until I have followed the history of these cells in *Petromyzon*.

I may here point out, however, that Kleinenberg§ appears to me to have been quite right when he suspected that the cells described by Mayer might be analogous to certain sub-umbrellar ganglion cells in the larva of *Lopadorhynchus*, which "introduce" the development of the ventral cord: and that, just as in the Annelid, the development of the vertebrate central nervous system would appear to have been initiated by a larval nervous apparatus outside the same. I propose to discuss this question in a future paper.

V. "The Assimilation of Carbon by Green Plants from certain Organic Compounds." By E. HAMILTON ACTON, M.A., Fellow of St. John's College, Cambridge. Communicated by W. T. THISELTON DYER, C.M.G., F.R.S. Received April 20, 1889.

(Abstract.)

The recent synthesis of a true glucose ("Acrose")‖ by Fischer and Tafel, and additions to our knowledge of the structure of dextrose and lævulose by Kiliani,¶ &c., seem to render desirable fresh experiments on the synthetical production of carbohydrate in green plants from sources other than CO_2 (*i.e.*, from organic compounds in which C is already combined with H and O).

* The homology of the giant ganglion cells described by Fritsch in *Lophius* is doubtful.

† *Vide* the excellent figure (fig. 143) in Hatschek's 'Lehrbuch der Zoologie,' p. 138. Probably *Amphioxus* possesses a transient or larval nervous apparatus.

‡ *Op. cit.*, p. 229.

§ N. Kleinenberg: "Die Entstehung des Annelids aus der Larve von Lopadorhynchus." 'Zeitschr. Wiss. Zool.,' vol. 44, pp. 220–221.

‖ 'Deutsch. Chem. Ges. Berichte,' vol. 20, pp. 1088, 2566, 3384.

¶ *Ibid.*, vol. 19, p. 221.

Papers have been published by A. Meyer and E. Laurent* dealing with this question.

A. Meyer has shown that the leaves of green plants can form starch when supplied with solutions of glucose, saccharon, mannite, inulin, glycerin. E. Laurent has confirmed this observation for glycerin.

A. Meyer has shown that starch is not formed from solutions of raffinose,† inosite, erythrite, dulcite, trioxymethylene, aldehyde (acetic); and Wehmer‡ that starch is not produced from formic aldehyde or formose.§

The method used by these observers‖ is placing leaves which have been deprived of starch by keeping in the dark in solutions of the substances, and testing for starch after a certain interval of exposure to daylight.

I have used "culture" experiments, and in most cases removed the starch at first present in tissues, not by keeping in the dark, but by placing in a receiver, the air of which is completely deprived of CO_2 by KOH and soda-lime.

Sachs's method of testing for starch was generally used, but in most cases also supplemented by direct microchemical observations.

The experiments fall mostly under three headings—

1. Experiments with shoots.
2. Experiments with entire plants, the carbon compounds being supplied to the roots.
3. Experiments with shoots of "water-plants."

In all cases the plants or shoots were placed in a "culture liquid" whilst being completely deprived of starch, and then transferred to another portion of the same culture solution to which the carbon compound had been added in known quantity. The "culture liquid" used was composed as follows:—

Distilled water	100 grams.	KNO_3	0·15 gram.
$MgCl_2$........	0·10 „	$Ca_3(PO_4)_2$	0·05 „
$FeSO_4$	0·025 „	$CaSO_4$	0·05 „

* Both in 'Botan. Zeitung,' 1886. Meyer's paper gives an account of previous work on this subject.

† Details as to constitution of these compounds and their relations to glucoses are given by Tollens, 'Handbuch der Kohlenhydrate,' Breslau, 1886.

‡ 'Deutsch. Chem. Ges. Berichte,' vol. 20, p. 2614.

§ Quite recently Fischer and Loew have independently shown that formose is a complex mixture, but that it contains a small quantity of a true glucose—probably "acrose." Loew states that this polymerisation only occurs with dilute solutions of the aldehyde ('Deutsch. Chem. Ges. Berichte,' vol. 22, 1889, Nos. 3, 4). Compare also on this point Tollens, *loc. cit.*, p. 250—252.

‖ *Vide* 'Botan. Zeitung,' *loc. cit.*

The shoots or plants in this liquid were placed under a bell-jar, so arranged as to exclude all possibility of entry of CO_2 from surrounding air during the experiments, but to allow a free circulation between the air in bell-jar and external atmosphere: this is effected by tubes introduced through the india-rubber stopper of bell-jar communicating with the outside through soda-lime U-tubes. Several capsules of soda-lime and cylinders of strong KOH solution were placed under the bell-jar to absorb any CO_2 given off by respiration.

In No. 2, special precautions were taken to prevent any CO_2 evolved from roots finding its way to the leaves.

In No. 3, the same result was obtained by the addition of barium acetate to the solution in which the plants were immersed. The apparatus was less complex in this case.

In all the experiments, provision was made that the air in confined spaces might be in free communication with external atmosphere (to admit free supply of oxygen), but always by passage through soda-lime U-tubes.

The plants used were not selected for any particular reasons beyond the fact that a good supply of them was conveniently at hand during the progress of these experiments. They were for No. 1, shoots (cut branches)—

Acer pseudo-platanus, L.
Ranunculus acris, L.
Tilia Europæa, L.
Alisma plantago, L.
Phaseolus vulgaris, L.
Cheiranthus cheiri, L.
Scrophularia aquatica, L.

For (No. 2) whole plants, seedling plants of—

Acer pseudo-platanus, L.
Phaseolus multiflorus, L.
Quercus robur, L.
Euphorbia helioscopia, L.
Phaseolus vulgaris, L.
Cheiranthus cheiri, L.
Campanula glomerata, L.
Epilobium hirsutum, L.

For (No. 3) water-plants, shoots of—

Anacharis alsinastrum, Bab.
Sparganium natans, Bab.
Chara vulgaris, L.
Callitriche aquatica, Sm.
Fontinalis antipyretica, L.

The substances (carbon compounds) used in all cases (1) and (2) in some cases also (3) were acroleïn, acroleïn-ammonia, acroleïn compound with acid sodium sulphite ($NaHSO_3.C_3H_4O$), allyl alcohol, glucose (partly quantitative), acetic aldehyde, aldehyde (acetic) ammonia, glycerin, lævulinic acid, calcium lævulinate, saccharon (cane-sugar, partly quantitative), inulin,* dextrins, soluble starch, glycogen,

* The inulin was free from glucoses, but it is doubtful whether it was really pure.

"extract of natural humus," the "humus-like" product obtained by the action of alkalis on saccharon. The results of these experiments may be tabulated as follows:—

Starch is formed when the compound is supplied either directly to the shoots or through the medium of the roots, with glucose, saccharon, glycerin,* inulin.† (A. Meyer's observation for "supplied to shoots.")

Starch formed when compound is supplied directly to the leaves, but *not* when supplied to the roots, with "soluble starch."

Starch formed when compound is supplied to the roots, but not when directly supplied to the leaves, with "extract of natural humus."

Starch not formed at all from acroleïn, acroleïn-ammonia, compound of acroleïn with $NaHSO_3$, allyl alcohol, aldehyde, aldehyde-ammonia, dextrin, glycogen, lævulinic acid, calcium lævulinate, "artificial humus substance."

That glucose is more readily taken up by the roots of plants from 0·5 per cent. solution than saccharon.

That the roots of plants can withdraw all the glucose from a 1 per cent. solution if they be left in the solution for a sufficient length of time, and the plants remain healthy.

The conclusions I should draw from these experiments are: That green plants cannot normally obtain carbon for assimilation from organic substances except carbohydrates or closely related bodies, not from aldehydes or their derivatives, and not even from all carbohydrates.

That a compound may be a source of carbon when supplied to the leaves but not when supplied to the roots, and *vice versâ*.

That green plants, owing to the normal process of obtaining carbon being from CO_2, have, to a large extent, lost the power of using organic compounds as a source of carbon. (Parasitic and saprophytic plants, especially fungi, undoubtedly do always obtain their carbon from complex organic compounds.)

That many (? all) green plants behave in the same manner towards the substances enumerated as regards formation or non-formation of starch. (Contrast in this respect with fungi, which are often characterised by decomposing special substances.)

That if a single substance of an aldehydic or ketonic nature is formed by plants as an intermediate product between CO_2 and H_2O and glucose (or starch), it can only be polymerised by the plant under special conditions, probably at the moment of formation.

* When glycerin was supplied to the roots in solutions stronger than 10 per cent. starch was not found.

† *Vide* note above.

VI. "Appendix to Paper* on Descending Degenerations following Lesions in the Gyrus marginalis and Gyrus fornicatus in Monkeys." By E. P. FRANCE. Communicated by E. A. SCHÄFER, F.R.S. Received April 22, 1889.

(Abstract.)

This appendix contains an account (1) of the degenerations which have resulted from lesions of the external motor cortex alone; (2) of the degenerations which have resulted from lesions involving both this and the gyrus marginalis; and (3) a comparison between the respective degenerations resulting from lesions of the external motor surface and of the gyrus marginalis. The results tend to show the existence of a differentiation in the pyramidal tract, the whole area of which is involved in degeneration when both external and mesial motor areas have been removed, whereas after removal of one portion only of the motor area, a corresponding part only of the pyramidal tract area degenerates.

VII. "On *Phymosoma varians*." By ARTHUR E. SHIPLEY, M.A., Fellow and Lecturer of Christ's College, Cambridge, and Demonstrator of Comparative Anatomy in the University. Communicated by A. SEDGWICK, F.R.S. Received May 1, 1889.

(Abstract.)

The following observations on *Phymosoma varians* were made on a number of specimens brought from the Bahama Islands by Mr. W. F. R. Weldon, of St. John's College, Cambridge. I am not only indebted to Mr. Weldon for the material of this article, but for very valuable suggestions and assistance while carrying on my investigations.

The *Phymosoma* is found embedded in soft coral rock. The length of the fully extended specimens averages 5 cm., the greatest diameter about 5 mm. The introvert is equal in length to the rest of the body.

The Head.—The head bears a crown of tentacles, which are always equal in number, usually eighteen; they are arranged in a horse-shoe shaped lophophore, which is dorsal to the mouth. The ends of this horse-shoe are confluent with those of a vascular lower lip, which is also horse-shoe shaped; the crescentiform opening between these two

* Read March 27, 1889.

structures is the mouth. The space included in the cavity of the tentacular horse-shoe—the representative of the pre-oral lobe—is covered with a peculiar pigmented epithelium, curiously wrinkled. This epithelium is continuous with the brain, and from it two sensory pits descend into that organ. The tentacles are short, the outer surface is grooved and ciliated, the inner surface covered with a pigmented epithelium, similar in character to that covering the pre-oral lobe.

The introvert immediately behind the head is smooth for about 2 mm. At the posterior end of this region a thin but very extensible collar is attached. The anterior end of the collar is free, and in specimens with the introvert inverted, completely covers the head. Behind the collar the introvert is surrounded by rings of hooks. Each hook is secreted by a multicellular papilla, and the ring rests upon a cushion formed by circular muscle fibres. Behind each ring of hooks is a parallel ring of sense organs, described below, and underneath each ring is a circular nerve, continuous with the ventral cord.

The Ectoderm.—The ectoderm covering the lower lip and the outside of the tentacles is ciliated, that covering the pre-oral lobe and the inside of the tentacle is crowded with brown pigment granules. The ectoderm of the rest of the body is one cell thick. It is curiously vaulted, leaving irregularly scattered spaces between it and the outside of the circular muscles, in which a nutritive fluid probably circulates. On its outer surface the ectoderm secretes a thick cuticle. From time to time some of the ectodermal cells are modified to form the skin papillæ. These are in the form of double cups, the inner lining of which consists of very large cells crowded with yellow spherules. The cup opens to the exterior by a minute pore. These papillæ are particularly large and numerous at the extremities of the trunk and on the dorsal surface. Those of the introvert differ from those on the trunk by the possession of a chitinous ring round their orifice.

General Anatomy.—There is nothing peculiar in the general anatomy of this Gephyrean as seen by the naked eye. Of the four retractors, the two ventral ones are much longer than the two dorsal, and at the base of the former is the generative organ. The intestine is supported by a spindle muscle, as well as by a muscle which passes from the ventral body wall to the anterior end of the visceral loop. There are about twenty-two longitudinal muscles in the middle of the trunk. The body cavity is lined by a flat epithelium, which is never ciliated.

The Skeletal Tissue.—I have given this name to a peculiar form of tissue which is found in the collar and tentacular crown of *Phymosoma.* The cells composing this tissue are roundish, with large nuclei. The protoplasm of the cells is traversed by numerous fine lines, and both it and the nuclei readily take up staining fluid. The cells are not so closely packed as to lose their shape.

This tissue forms a ring in the lower lip, external to the vascular spaces in this region; it also sends extensions into the tentacles. It seems to support and stiffen these structures, and from its position serves as a firm hold for the insertion of the retractor muscles of the introvert, which are attached just behind it.

The Alimentary Canal.—The œsophagus is lined with ciliated epithelium, continuous with that of the lower lip and the tentacles. Its cavity is diminished by numerous ridges, the grooves between which are continuous with the grooves on the tentacles. The intestine forms about fifteen coils; it is usually full of fine sand, and appears to be partly ciliated, though there is no ciliated groove as in *Sipunculus.* The rectum is straight, and its cavity is also occluded by ridges. The anus is surrounded both by a sphincter muscle and by a number of radiating fibres.

The Vascular System.—There are two kinds of blood corpuscles present in *Phymosoma.* The larger kind occurs in the body cavity: they are oval in outline, with a spherical nucleus. The smaller kind is found in a closed series of spaces usually termed the vascular system. This space may be described as consisting of three parts, all communicating with one another. The first of these lies in the lower lip, and consists of a number of channels anastomosing with one another, the interspaces being occupied by the skeletal tissue described above. These channels open at their dorsal ends into the second series, which forms a space at the base of the lophophore. This gives off a number of spaces, which pass into the tentacles and open in the middle ventral line into the dorsal vessel. This is a muscular sac which is attached to the dorsal surface of the œsophagus between the retractor muscles of the right and left side. It serves as a reservoir, into which the corpusculated fluid is driven when the introvert is retracted and the tentacles are flaccid. By the contraction of its wall the blood is forced into the lower lip and lophophore, and these organs are then dilated. The whole is lined by a flat epithelium.

The Nephridia.—Each nephridium consists of two parts, the bladder and the true secreting part. Both these parts are well supplied with muscle fibres and are consequently very contractile, so that their shape varies greatly in different specimens. The bladder, which is the anterior half, is attached to the body wall by mesenteries, the secreting part is free. The bladder opens to the exterior by a circular mouth, and to the interior or body cavity by a ciliated opening in shape like a flattened funnel. The lumen of the secretory part is broken up into a number of side chambers, which may be readily described by comparing them to the air-chambers in the interior of a frog's lung. The whole is lined by a very peculiar epithelium. The cells composing this are columnar in shape, with their nuclei at the base. They are crowded with minute spherical granules, and many

of them have at their free end a bubble or vesicle in which these granules have accumulated. From time to time these vesicles break off and lie in the lumen both of the secretory part and of the bladder, and are no doubt extruded from the body. The whole process is very like the excretion of milk in a mammary gland. The only other structures, besides these vesicles, found in the lumen of the nephridia are the generative cells, ova and spermatozoa; it is remarkable that the cœlomic corpuscles never enter them.

The Nervous System.—The brain is a bilobed organ lying immediately beneath the pre-oral lobe, with the ectoderm of which it is continuous. The ventral and posterior surfaces project into a blood sinus which is situated in the neighbourhood of the junction of the lophophoral vessels with the dorsal vessel. The ganglia in the brain form a cap on the anterior, dorsal, and posterior surfaces, enclosing the fibrous tissue which comes to the surface of the brain ventrally. The ganglion cells are mostly small and bipolar, but on the posterior surface are a certain number of unipolar giant ganglion cells, with a diameter at least four times that of the smaller cells.

The brain gives off three pairs of nerves; at the side a pair of circumœsophageal commissures which pass round the œsophagus and fuse to form the ventral nerve cord. This has no trace of a double origin or of segmentally arranged ganglia, but from time to time it gives off a nerve which passes into the body wall and there splits into a right and left nerve; these reunite in the dorsal middle line and so form a nerve ring. These nerve rings are especially conspicuous in the skin of the introvert. The second pair of nerves given off from the brain pass into the base of the lophophore, and give off a branch into each tentacle, where it lies immediately beneath the ciliated groove. The third pair, which arise nearest the median line, pass to supply the skin of the pre-oral lobe.

The sense organs are of two kinds: (i) sensory pits in the brain, (ii) ectodermal sense organs in the introvert. The former are two pits which open on to the pre-oral lobe, and end blindly in an expanded vesicle in the substance of the brain. They are lined with columnar epithelium, which at the inner end is crowded with dark-brown pigment. Their lumen sometimes contains a clot. The latter are groups of ectodermal cells, which have increased in size and at their outer end are provided with short stiff processes which project above the general level. These are gathered together into a small brush by a chitinous ring which surrounds their base. These organs occur in rings at the base of the rings of hooks in the introvert.

The Reproductive Organs.—*Phymosoma varians* is diœcious. In both sexes the reproductive organs form fimbriated ridges which are attached to the bases of the ventral retractor muscles, and are continuous across the interspace between these two muscles, ventral to

the nerve cord. The cells forming these ridges are continuous with the peritoneal lining of the body wall, and in this region the elsewhere flattened epithelium has become modified to form the ova in the female and the mother-cells of the spermatozoa in the male. The ridge is thickened at its free end, and here the more mature generative cells are found. When ripe these dehisce into the body cavity in which they live for some time and increase in size. The ova found in the cœlomic fluid are oval in outline, their nucleus is very large, and they are surrounded by a thick zona radiata. No ripe spermatozoa are found in the cœlom, but their mother-cells exist in various stages of division.

Conclusions.—The more important contributions to the anatomy of the Gephyrea contained in the foregoing Abstract are the descriptions of (i) the head with the extensible collar, (ii) the skeletal structures, (iii) the minute anatomy of the nephridia, and (iv) the histology of the nervous system and sense organs.

The most important conclusions to be drawn from these facts seem, in my opinion, to confirm those systematic writers who assign *Phoronis* a position in the immediate neighbourhood of the unarmed Gephyrea. In addition to the points upon which they lay stress in the general anatomy of these forms, I would point to the close similarity in structure and position of the skeletal tissues in both groups, and the possible homology between the collar in *Phymosoma* and the extensible calyx which surrounds the head in *Phormis*.

VIII. "On the Dentition of *Ornithorhynchus*." By OLDFIELD THOMAS, Natural History Museum. Communicated by Dr. GÜNTHER, F.R.S. Received May 8, 1889.

[PLATE 2.]

At the meeting of the 9th of February, 1888,* Mr. E. B. Poulton communicated to this Society the first discovery of the presence of teeth in *Ornithorhynchus*, a discovery which naturally awakened extreme interest throughout the scientific world. This first account was afterwards elaborated into a long and excellent description† of the form, structure, and development of the teeth, and their relationship to the horny plates (or "cornules," as they may be conveniently termed) which form the functional masticatory organs of the adult animal.

Apart from the valuable histological descriptions, the conclusions put forward in Mr. Poulton's paper may be briefly epitomised as follows:—

* 'Roy. Soc. Proc.,' vol. 43, 1888, p. 353.

† 'Quart. Journ. Microsc. Sci.,' vol. 29, 1888, p. 9.

1. Teeth are present, and they are typically mammalian in form and structure.

2. They are apparently functionless, being mere persistent rudiments of teeth functional at an earlier stage of the animal's history, and, judging from the material examined, are absorbed without ever cutting the gum.

3. The cornules are developed from the buccal epithelium covering the teeth, and take their first shape from the dental cusps and ridges beneath them.

The grand fact of the presence of teeth in Monotremes, and their mammalian nature, are discoveries on which Mr. Poulton may well be congratulated, but, owing to the extreme youth of his specimens, the inferences placed under the headings 2 and 3, well founded as they then appeared to be, prove to require some modification.

Thanks to the material before me, I am now enabled to carry forward our knowledge of the development of the teeth in their later stages with somewhat greater certainty, but the fact that this knowledge has not been previously obtained affords a striking instance of our ignorance of the most obvious and easily discovered points in mammalian morphology.

Re-stated according to the present observations, the paragraphs above referred to should be—

2. The teeth are functional for a considerable part of the animal's life, cutting the gum as usual, and, after being worn down by friction with food and sand, are shed from the mouth as are the milk teeth of other mammals.

3. The cornules are certainly developed from the buccal epithelium, but from that *under** *and around*, instead of over, the teeth, and the hollows in the plates are the vestiges of the original alveoli of the teeth, from out of which the latter have been shed.

The material on which these conclusions are based is as follows:—†

a. The skull, extracted from the skin, of a young male *Ornitho-*

* The idea of epithelium *under* a tooth seems at first sight an absurdity, since of course the tooth is originally formed under the epithelium, and cuts through it on being exserted. In this case, however, as a later stage, the epithelium appears to push its way underneath the tooth, and, as the roots are absorbed, to obliterate the holes in which they were situated, and, after the loss of the teeth, to form the even surface of the hollows in the cornules.—May 17, 1889.

† Since the above was written, Professor Stewart, of the College of Surgeons, has discovered a set of teeth in a specimen in that museum. Of these teeth, ten instead of eight in number, he is now preparing an account. Many details will be found there as to the development and histology of the teeth and cornules that could not be made out in the Museum specimens. I must at the same time express my obligations both to Professor Stewart and Mr. Poulton himself for much advice and assistance in making out the history of the teeth and cornules.—May 17, 1889.

rhynchus just over a foot in total length, and therefore probably about one-third grown.

b. That of a young female preserved in spirit, slightly smaller, but, as being a female, in rather a later stage of development than *a*. This skull is just 65 mm. in basal length.

Skull *a* was unfortunately cleaned before any suspicion arose that it was of such special interest, and in the maceration, the teeth, with the exception of those on the left side of the mandible, fell out and have had to be replaced, so that there is a slight element of uncertainty as to their exact positions.*

In this skull, both above and below, we find on each side two† large and completely calcified teeth (B and C of Mr. Poulton's figures and descriptions), surrounded and separated from each other by a narrow rim of indurated epithelium, which evidently had been (as is proved by specimen *b*) continuous with the ordinary lining epithelium of the mouth. Beneath the teeth are separated from the bone by an incomplete layer of the same material.

The teeth themselves (Plate 2, figs. 1 and 2) are broad, flat, and low-crowned. The upper ones have each two high, conical, internal cusps, from which minute ridges run downwards and outwards to the outer borders of the crowns, where the edge is peculiarly crenulate rather than cuspidate, in the ordinary sense of the word. On the whole, the anterior and posterior upper teeth are essentially similar to one another, except that the former are narrower, and their outer edges are less markedly crenulated.

In the lower jaw there is a greater difference between the two. The anterior (fig. 2) is triangular in outline, its longest side is placed antero-externally, and its anterior and postero-external angles have each a high pointed cusp,‡ ridged on its internal aspect, while the posterior and internal borders are indistinctly crenulated. The posterior tooth is broadly quadrangular in outline, with a projecting antero-internal angle. As in the corresponding tooth above, there are two cusps on one side, and a series of crenulations on the other, but they are of course reversed, the cusps being external and the crenulations internal. The cusps are high, and connected with transverse ridges running across towards the internal border. The crenulations on the internal border are more numerous in the present specimen than in that figured by Mr. Poulton, there being nine instead of five minute projections.§

* This doubt is dispelled by an examination of the College of Surgeons specimen, in which the teeth correspond precisely with those of the British Museum skull as now placed.—May 17, 1889.

† Three on each side below in the College of Surgeons specimen.—May 17, 1889.

‡ These are evidently the two cusps shown commencing to calcify in Mr. Poulton's figure. (Plate II, fig. 16, B. *a.o.c.* and *p.o.c.*)

§ A considerable amount of individual variation is to be expected in the case of

On their under surfaces the teeth are rough, and show traces of the presence of minute roots, which have apparently been already partly absorbed.*

The actual dimensions of the teeth are as follows:—

	Length.	Breadth.
Upper anterior............	4·0 mm.	2·3 mm.
„ posterior...........	4·0 „	2·6 „
Lower anterior	3·5 „	3·2 „
„ posterior...........	4·2 „	2·7 „

Turning now to specimen *b*, we find that it presents a most interesting state of affairs, although had the animal lived but a day or two longer, all its interest for our present purpose would have vanished, for it has all but completed the process of shedding its teeth. On opening the mouth there was seen the usual set of hollows, surrounded by hardened epithelium (fig. 3), characteristic of the first stage in the development of the cornules. Two large hollows were to be seen on each side of each jaw, and a minute additional one in front above, and behind below, these hollows being of course afterwards the concavities on the surface of the cornules. The hollows were all filled up nearly level with the surface with bits of earth and sand, and fragments of food. On cleaning this out bit by bit, two† of the eight large hollows were found to contain something in addition, and a close examination proved that this something was in each case a worn-down tooth, reduced to about the thickness of paper, and with all the outlines worn off. These remnants of teeth were quite unattached, coming away freely, and would evidently very soon have fallen out of their own accord. No epithelium was over them, but all that surrounding and beneath them was commencing to indurate and thicken, in order to form what would later have been the cornule.

These two specimens, therefore, prove the contentions put forward above; *a* shows that the teeth are functional, completely calcified, and placed as usual close to the bone; *b* that, after being worn down by genuine use, they are shed from the hollows in the surface of the cornules, which grow up beneath and around instead of being formed above them. The specimens examined by Mr. Poulton were all from animals far younger than in the case of those now described; so far younger, in fact, that instead of being at, or nearly at, their furthest

organs in such a state of decadence as are the teeth of *Ornithorhynchus*, a view that is borne out by the marked differences between the teeth now described and those of the College of Surgeons specimen.—May 20, 1889.

* This suggestion is confirmed by Professor Stewart's specimen, in which the teeth have well-defined roots.—May 17, 1889.

† The anterior right and the posterior left of the upper jaw.

point of development, as was then not unnaturally supposed, they were merely commencing to undergo calcification within the tooth-capsule, just as would have been the case with those of any other young mammal.

But in some ways the point that is of most importance in the discovery of fully-developed Monotrematous teeth is the fact that for the purpose of comparison with those of other mammals, a comparison to which of late great attention has been directed, we have now available perfect calcified teeth, of a size sufficient for inspection with the naked eye, and very far superior to anything that figures compiled from microscopic sections can possibly be.

Such a comparison I would have willingly now made, but unfortunately the most careful search* among other animals, fossil and recent, mammalian and reptilian, fails to reveal any teeth quite corresponding to those of *Ornithorhynchus*. But, nevertheless, their study inclines one more and more to believe in the correctness of Professor Cope's† ingenious suggestion as to the Monotrematous, or, as I should prefer to say, Prototherian, nature of the Mesozoic‡ Multituberculata. These animals, long looked upon as Diprotodont Marsupials, have of late been much studied in America,§ where large numbers of them have been found. Many of them (*e.g.*, *Bolodon*, *Allodon*, *Ptilodus*, and, especially, the best known of all, *Microlestes* and *Plagiaulax*) have molar teeth which are broad and low-crowned, and which have a series of cusps running around their edges, so that each tooth has two rows of cusps corresponding in a general way to the cusps on one side and the crenulations on the other in the teeth of *Ornithorhynchus*. A figure of one of the molar teeth of *Microlestes* is given (fig. 5) to show how far it resembles those of the living form.

Still it must be insisted that the resemblance between the Multituberculate- and the Ornithorhynchus-teeth is of the most general character, and that the two are certainly widely separated genetically, even if we do admit that they appear to possess a relation-

* In this search I have had the advantage of the assistance of Mr. R. Lydekker and Mr. G. A. Boulenger.

† 'Amer. Nat.,' vol. 22, 1888, p. 259. Professor Seeley's remarks in 1879 ('Quart. Journ. Geol. Soc.,' vol. 35, p. 456 *et seq.*) on the relationship to the Monotremata presented by a Mesozoic humerus and femur assigned either to *Phascolotherium* or *Amphitherium* do not touch the question, since neither of these animals are Multituberculata, both belonging to the Polyprotodont division of the Mesozoic mammalia.—May 17, 1889.

‡ The Eocene *Neoplagiaulax*, Lemoine (Paris, 'Soc. Géol. Bull.,' vol. 77, p. 249), also belongs to this group, and has teeth that present a certain resemblance to those of *Ornithorhynchus*. Compare Plate V, fig. 3, and VI, figs. 17–19, of that work with the figures now given.

§ See papers by Cope, Marsh, Osborn, Scott, and others.

2

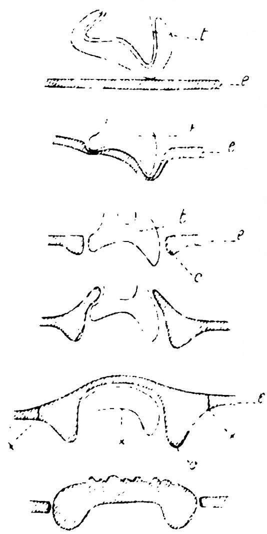
× 5

5.

× 10

1

4. × 4



6.

3. × 4

ship nearer to each other than to any other known groups of mammals.

In any case, since the form and structure of the teeth are of necessity the chief means of determining the evolutionary history of the Mammalia, the discovery now made, in giving us genuine modern Monotrematous teeth to work from, provides one of the most important aids to the elucidation of the systematic position of these anomalous mammals that has yet been obtained.

Finally, it may be noted that the absolute continuity of the epithelium with the developing cornule, combined with the presence of such well-developed calcified teeth, proves again, if after Mr. Poulton's paper further proof is needed, that the view* as to the cornules being degenerated true teeth is wholly untenable.

EXPLANATION OF PLATE 2.

Fig. 1. Left upper teeth of *Ornithorhynchus* (× 5). Drawn from specimen *a* (see *suprà*); the rim of indurated epithelium still present.

2. Left lower teeth (× 5).
3. Left side of palate of specimen *b*, showing (*a*) the empty anterior alveolar hollow, (*b*) the worn-down posterior tooth not yet shed, and (*c*) the elevated rims of epithelium that would later have formed the walls of the cornules.
4. The same in the lower jaw, but here all the teeth have been shed: *c* as in 3.
5. Molar tooth of *Microlestes*, much magnified.
6. Series of diagrammatic sections showing development of tooth and cornule.†
   - *a*. Tooth (*t*) still in capsule below gum; *e*, epithelium. This sketch is taken from one of Mr. Poulton's figures of the early stages of the teeth.
   - *b*. Tooth just before eruption.
   - *c*. Eruption of tooth and consequent cutting of the epithelium, which commences to thicken at *c* for the formation of the cornule.
   - *d*. Creeping of epithelium underneath tooth, until it presses against and gradually causes absorption of their roots.
   - *e*. Tooth just before it is shed. Its roots have been absorbed, and the epithelium has passed right beneath it. The cornule is now definitely separated (at *x*) from the ordinary epithelium, and its edges are so developed as to overtop the cusps of the tooth.
   - *f*. Fully developed cornule.

---

* Seeley, 'Roy. Soc. Proc.,' vol. 44, 1888, p. 129.

† These diagrams were drawn up in consultation with Mr. Poulton, so that he is equally responsible for them with myself.

Presents, May 16, 1889.

Transactions.

Boston:—Society of Natural History. Proceedings. Vol. XXIII. Parts 3–4. 8vo. *Boston* 1888. The Society.

Edinburgh:—Royal Society. Proceedings. Vol. XV. No. 128. Vol. XVI. (Pp. 65–128.) 8vo. *Edinburgh* 1889. The Society.

London:—Quekett Microscopical Club. Journal. Vol. III. No. 24. 8vo. *London* 1889. The Club.

Society of Biblical Archæology. Proceedings. Vol. XI. Part 6. 8vo. *London* 1889. The Society.

Zoological Society. Transactions. Vol. XII. Part 8. 4to. *London* 1889; Proceedings. 1888. Part 4. 8vo. *London* 1889. The Society.

Lund:—Universitet. Ars-Skrift. Tom. XXIV. 4to. *Lund* 1887–88. The University.

Milan:—Reale Istituto Lombardo di Scienze e Lettere. Memorie (Classe di Lettere e Scienze Morali e Politiche). Vol. XVIII. Fasc. 1. Memorie (Classe di Scienze Matematiche e Naturali). Vol. XVI. Fasc. 2. 4to. *Milano* 1887–88. The Institute.

Newcastle-upon-Tyne:—North of England Institute of Mining and Mechanical Engineers. Transactions. Vol. XXXVIII. Parts 1–2. 8vo. *Newcastle* 1889. The Institute.

Paris:—École Normale Supérieure. Annales. Année 1889. No. 4. 4to. *Paris.* The School.

Société Philomathique. Bulletin. Tome I. No. 1. 8vo. *Paris* 1889. The Society.

Rome:—Accademia Pontificia de' Nuovi Lincei. Atti. Anno XXXIX. Sessione 2–4. 4to. *Roma* 1886. The Academy.

R. Comitato Geologico d' Italia. Bollettino. 1889. Nos. 1 e 2. 8vo. *Roma.* The Comitato.

St. Petersburg:—Académie Impériale des Sciences. Mémoires. Tome XXXVI. Nos. 12–13. 4to. *St. Pétersbourg* 1888–89. The Academy.

Journals.

American Journal of Philology. Vol. IX. No. 4. 8vo. *Baltimore* 1888. The Editor.

Annales des Mines. Sér. 8. Tome XIV. Livr. 6. 8vo. *Paris* 1888. L'École des Mines, Paris.

Glasgow Medical Journal. General Index. 1828–88. 8vo. *Glasgow* 1889. Glasgow and West of Scotland Medical Association.

Horological Journal. May—December, 1888. January—May, 1889. 8vo. *London.* British Horological Institute.

Journals (*continued*).

Medico-Legal Journal. Vol. VI. No. 3. 8vo. *New York* 1888.
The Editor.

Naturalist (The). Nos. 165–166. 8vo. *London* 1889.
The Editors.

Revista do Observatorio. 1889. Num. 2–3. 8vo. *Rio de Janeiro.*
Imperial Observatory, Rio de Janeiro.

Revue Médico-Pharmaceutique. 1889. No. 2. 4to. *Constantinople.*
The Editor.

Stazioni Sperimentali Agrarie Italiane (Le). Vol. XV. Fasc. 3. Vol. XVI. Fasc. 1–3. 8vo. *Roma* 1888–89. The Editor.

Technology Quarterly. Vol. II. No. 3. 8vo. *Boston* 1889.
The Editors.

Zeitschrift für Naturwissenschaften. 1887. Hefte 3–6. 1888. Hefte 1–4. 8vo. *Halle.*
Naturwissenschaftlicher Verein, Halle.

"On the Limit of Solar and Stellar Light in the Ultra-violet Part of the Spectrum." By William Huggins, D.C.L., LL.D., F.R.S. Received March 28,—Read April 4, 1889.

It has been long known that the solar spectrum stops abruptly, but not quite suddenly, at the ultra-violet end, and much sooner than the spectra of many terrestrial sources of light. The observations of Cornu, of Hartley, and, quite recently, of Liveing and Dewar, appear to show that the definite absorption to which the very rapid extinction of the solar spectrum is due, has its seat in the earth's atmosphere, and not in that of the sun; and that, consequently, all ex-terrestrial light should be cut off at the same place in the spectrum.

During several years I have attempted to obtain the limit in the ultra-violet for stellar light here, but as it was necessary to make use of a bright star at a high altitude, and at a time when the atmosphere was very clear, it was not until September 20th, 1888, that I was able to obtain a result which seemed to me to be satisfactory.

On that night three successive photographs of Vega, with increasing exposures, were taken on the same plate. The first spectrum was exposed for 10 minutes, the second for 20 minutes, and the third spectrum nearly four times as long, namely, for 70 minutes.

A comparison of the extent of the second spectrum due to an exposure of 20 minutes with that of the third spectrum, to which an exposure of 70 minutes was given, leaves no doubt that the latter spectrum has reached the limit imposed by atmospheric absorption, and has not stopped in consequence of an insufficient exposure of the plate.

The original plate has been enlarged about four times; and a spectrum of magnesium and calcium, taken with the same apparatus, and enlarged simultaneously with the plate of stellar spectra, has been placed above to serve as a scale.

As the spectra are prismatic it is not possible to indicate the wave-lengths in a scale of equal parts. A short scale only is placed over the spectrum where the light of Vega ends.

The spectroscope with which the spectra were taken is furnished with a prism of Iceland spar and lenses of rock crystal, and a mirror of speculum metal was used to condense the light of Vega upon the slit.

It will be seen that at my observatory* the light of Vega at about λ 3000 is abruptly weakened, and then continues as a very faint line to the point of apparent extinction at λ 2970.

Numerous solar spectra taken here during the last four years with the same spectroscope show an average abrupt weakening at about λ 3000, and an apparent total extinction at about λ 2985.

On two occasions only the very faint weakened spectrum could be traced as far as λ 2970.

The abrupt narrowing of the spectrum at the end towards the red is produced by the rapid falling off of sensitiveness of the silver bromide for light of increasing wave-length.

The increase of breadth of the spectra with increase of duration of exposure is due to the same causes, optical and photographic, which produce the increase of diameter of stellar disks on the photographic plate with longer exposures, when a reflector is used. At *h* the breadths of the spectra, having 20 minutes and 70 minutes exposure respectively, are 0·06 inch and 0·105 inch.†

In 1879 Cornu‡ made experiments on the limit of the solar spectrum with reference to the altitude of the place of observation. On the Riffelberg, at an elevation of 8414 feet, the spectrum reached to λ 2932, while at the lower elevation of Viège, 2163 feet, the spectrum stopped at 2954. He concludes that the absorption is due to the gaseous constituents, and not to aqueous vapour in the atmosphere.

In 1881§ Hartley stated that an amount of ozone proportional to

* Elevation of the observatory 177 feet above mean sea level. Barometer about 30·03 inches at the time of observation.

† The law of increase of size of image with exposure is not as yet accurately defined. Bond found that the diameter of star-disks varied nearly as the square root of the time of exposure. Pritchard, using a reflector, found a law near the fourth root; and Mr. H. H. Turner has recently found a law very near the cube root for plates taken with a photoheliograph object-glass ('Astron. Soc. Month. Not.,' vol. 49, p. 292).

‡ "Sur l'Absorption Atmosphérique des Radiations Ultra-violettes," 'Journ. de Physique,' vol. 10, 1881.

§ "On the Absorption Spectrum of Ozone, and on the Absorption of Solar Rays by Atmospheric Ozone," 'Chem. Soc. Journ.,' vol. 39, 1881, pp. 57, 111—129.

the average quantity in a vertical column of the atmosphere, caused an absorption similar to that observed in the solar spectrum, namely, terminating about λ 2950.

Quite recently Liveing and Dewar have made some important experiments on the absorption-spectrum of large masses of oxygen under pressure.* They state that with a tube 165 cm. long and a pressure of 85 atmos., oxygen appeared to be quite transparent for violet and ultra-violet rays up to about λ 2745. From that point the light gradually diminished, and beyond λ 2664 appeared to be wholly absorbed.

In some later experiments with a steel tube 18 metres long and a pressure of 90 atmos., oxygen produced complete absorption above P, *i.e.*, λ 3359·2.

M. Janssen, from his observations on the Alps, concludes that both the bands which follow the law of the square of the density, and the dark lines obeying a different law of formation, which are due to oxygen in the solar spectrum, are produced exclusively by the earth's atmosphere—" L'atmosphère solaire n'intervient pas dans le phénomène."†

SPECTRA OF VEGA
showing the limit of stellar light in the ultra violet region

* 'Chemical News,' vol. 58, p. 163, and 'Phil. Mag.,' September, 1888, pp. 286—290.

† 'Comptes Rendus,' vol. 107, p. 677.

"The Specific Resistance and other Properties of Sulphur."

By James Monckman, D.Sc. Communicated by Professor J. J. Thomson, F.R.S. Received November 10,—Read December 6, 1888.

Resistance.

It is well known that sulphur in a solid state insulates electricity of very high potential, and conducts heat badly; also that it undergoes a curious series of changes when heated—melting at about 120° C., becoming thicker at 200° to 250°, more liquid at 250° to 300°, and boiling under atmospheric pressure at 440°. During the past three terms I have been engaged in the Cavendish Laboratory in trying to determine whether these changes are accompanied by corresponding ones in the electrical resistance and other properties of the element.

I expected that the changes would be within the limits of an insulating body, hence my first experiment was designed to test the insulating power for frictional electricity.

Two platinum wires were placed in a beaker of melted sulphur at a distance of about 1 cm. apart, one being connected to an electroscope, the other going to earth. When the sulphur became solid the leaves of the electroscope remained open on charging for a considerable time, but fell at once if any portion of the sulphur between the wires was liquid. To avoid all discharging by the flame used in melting the sulphur, the platinum wires were fixed to an ebonite rod at the proper distance apart. After melting the sulphur the flame was removed to a distance and the wires placed in the liquid. The discharge was complete and, apparently, as sudden as when contact was made with a wire.

The same experiment was tried with paraffin, and the discharge found to be very slow.

Seeing then that the resistance was removed by melting the sulphur into the region of conductors, it became necessary to find some method that could be used for conductors of very high resistance.

At Mr. Glazebrook's suggestion I tried placing the wires in melted sulphur in circuit with a high resistance reflecting galvanometer and a set of accumulators giving a total electromotive force of 60 volts. With platinum wires in the sulphur no reliable results could be obtained as the current quickly fell away.

While thus engaged my attention was directed to a paper by M. Duter ('Comptes Rendus,' vol. 106, 1888, p. 836), in which he describes some experiments on boiling sulphur. Platinum he found to be attacked by the sulphur, but gold gave good results; no measurements were, however, given. Following Duter's plan I used

gold electrodes, but failed to get a steady current to pass; neither did ordinary carbon plates answer any better.

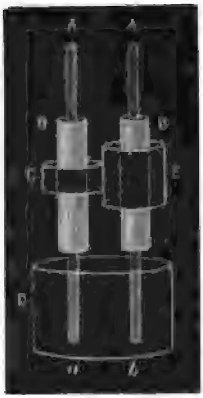
Having some graphite rods which had been procured from Hogarth and Hayes, Keswick, for some experiments on carbon, I tried two of them, and obtained a perfectly constant flow of electricity even at the boiling point. The change in the resistance between melting point and boiling point was so great that it was difficult to arrange a method that would give reliable readings. In the Cavendish Laboratory, where this work was done, we have a set of 26 accumulator cells which when charged give a potential of about 60 volts. This is conducted to all the rooms, and is so arranged that we can use any number of cells, so that we can vary the potential from 60 volts to about $2\frac{1}{3}$ volts.

When the sulphur was melted (125° C.) 60 volts gave a deflection with a reflecting galvanometer of 11,770 ohms resistance of only half a millimetre on the scale, while at 440° C. one cell gave a deflection of 60 mm.

By changing the number of cells, and measuring the potential by a Thomson's graded voltmeter at each change, the results given in the following tables were obtained.

The graphite rods were carefully insulated from each other by hard glass tubes, over which shorter pieces of tube of unequal length were placed, and fixed with plaster of Paris, the object being to give as much insulating surface as possible. A cell of mica was placed around the projecting part of the graphite rods to render the path of the current fairly parallel; the ends were left open to allow free access of sulphur and to prevent vapour taking the place of the liquid when ebullition commenced.

The form of the mica cell was preserved by an outer cell of thin glass.



In the figure, A, A are the graphite rods, B, B glass tubes, C, C

larger ones, D mica. The exposed ends were filed flat, and fit loosely into the tube D. The dimensions are: length, 30 mm.; width, $3\frac{3}{8}$ mm.; distance, 7 mm. between the free ends, *a*, *b*.

The change of resistance was so great and fell so quickly at the higher temperatures that it gave rise to a suspicion that the increase of conductivity might arise from particles of the rods being torn off and mixed with the sulphur. To test this, the rods were placed in a second tube of boiling sulphur, so that when placed in the tube of pure sulphur they might not reduce the temperature of the liquid. After removing and allowing the adhering liquid to flow back, they were placed in the other vessel, and the current passed at once. The deflection was the same as before, and steady, neither did the rods appear to be acted upon in any way.

Hence we concluded that the change was in the sulphur itself.

Resistance.—Ordinary roll brimstone :—

Temperature.	Potential.	Deflection.	Reduced to one standard by multiplying by 60/*v*.
440°	$2\frac{1}{8}$	60	1560
300	"	12·5	325
270	"	2	52
250	"	1	26
250	55	10	24
223	..	3	7
195	60	6	6
150	"	5	5
140	"	2	2
125	"	0·5	0·5

Another specimen gave slightly different numbers, but of the same order.

With precipitated sulphur the resistance was considerably higher.

440°	4·1	35	512
350	"	5	73
335	..	3	44
335	8·2	7	51
290	54	5	5·5
280	"	2·5	2·8
260	"	0·5	0·55
125	..	could get up a swing only.	..

The specific resistance may be calculated from these tables, using the quantities previously given, namely, l. 30 mm., w. $3\frac{3}{8}$, d. 7; and

the value of each scale division is $0{\cdot}03\dot{3}$ milliampère of current passing per second. Hence since $E/R = c$ we get—

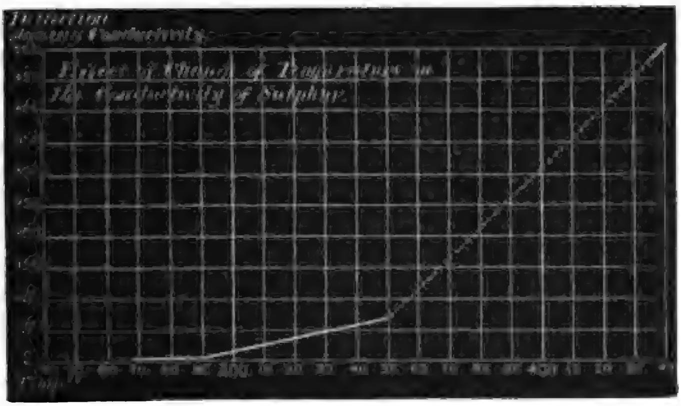
$$\frac{4{\cdot}1}{R} = 0{\cdot}0000011\dot{6} \text{ ampère}$$

$$R = 3{,}553{,}448 \text{ ohms.}$$

Specific resistance $= R \times \dfrac{30 \times 0{\cdot}3\dot{6}}{0{\cdot}7} = 1{\cdot}57 \times R = 5{,}600{,}000$ nearly, or 5×10^5 ohms at 440°; at 260° it is nearly 1000 times that number, or 5×10^8.

Roll sulphur gives the same resistance at 125°, that is, 5×10^8; while at 440° it is one-third precipitated sulphur, or $1{\cdot}\dot{6} \times 10^5$.

The accompanying curve shows the conductivity of precipitated sulphur. At 290° C. there appears a sharp bend in the line. Up to that point the conductivity rises to 5·5, becoming 51 in the next 45°, after which it rises rapidly. This bend coincides fairly with the second fluid state, and probably indicates some molecular change which appears to produce similar irregularities in its other properties.

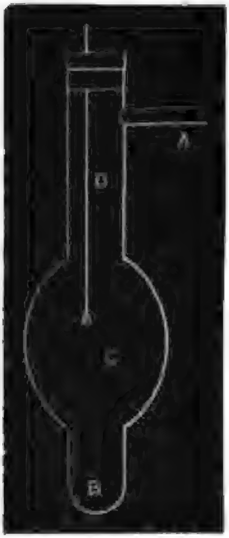


Boiling Point.

The first of these tested was the effect of pressure upon the boiling point.

The apparatus used is given in the next diagram, the sulphur being placed in the space B, whilst the larger spaces, C and also D, were surrounded with sheet asbestos to prevent rapid radiation. The tube A was connected with an air-pump and mercury gauge.

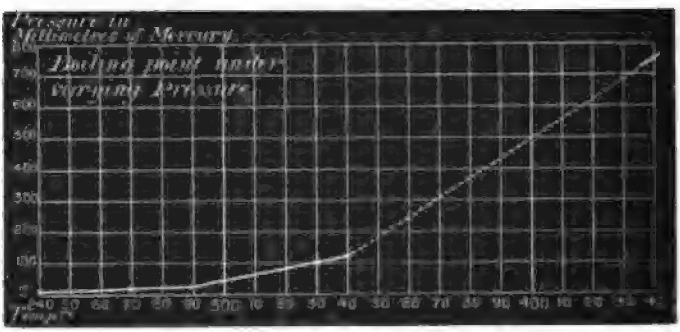
When the sulphur in B was heated, on exhausting the air the first time there was a violent temporary evolution of gas at 240° C. if the pressure was 23 cm.; but if the air was withdrawn until the gauge registered 1 mm., this ebullition took place at 150° C. This escape of



gas took place even after the sulphur had been boiled at the ordinary pressure of the atmosphere, and also after a considerable evolution at 150° when, apparently, all gas was driven away, a second one occurred on raising the temperature.

One or two precautions are necessary in order to get good results. The sulphur is a bad conductor of heat, and therefore one part of it near the flame may be many degrees above another in the interior. The vapour from the hotter bursts through the cooler liquid and rising into the space C causes a higher temperature to be registered than the true one. By reducing the flame, and by surrounding the portion B with a conductor such as mercury for temperatures below 350°, the heat can be regulated and spread until this is avoided.

The curve is drawn from data furnished by experiments at various temperatures up to 340° only, one temperature being taken above that,



namely, 440°. There appears the same change at 290° as in the previous curve which it closely resembles.

At one time I thought that sulphur might be added to the list of bodies given by Ramsay and Young in their paper on "Evaporation and Dissociation" ('Phil. Trans.,' 1884, p. 461) as a means of obtaining a range of temperature above 350° up to 440°. The chief difficulty arises from the overheating, before mentioned, and the danger of breaking a vessel of solid sulphur on reheating. The first can be avoided by carefully heating, and the second is very much reduced when the sulphur is allowed to solidify under much diminished pressure.

The two curves given above are so nearly identical that one naturally suspects that the former is produced by the increased mobility indicated by the latter, and that if the measurements for resistance were taken at each temperature when the liquid was under pressure so diminished that ebullition took place, the mechanical agitation of the particles would produce a decrease of the resistance in addition to that due to the temperature alone, and carry off the charge somewhat after the manner of air and a pointed conductor in electricity of high potential.

That this was not so was proved by placing the graphite rods in the vessel used for the last experiments. After heating and exhausting to expel the gas measurements were taken at various temperatures; in one set the sulphur boiled under diminished pressure, and in the other set the air was admitted. No difference could be detected.

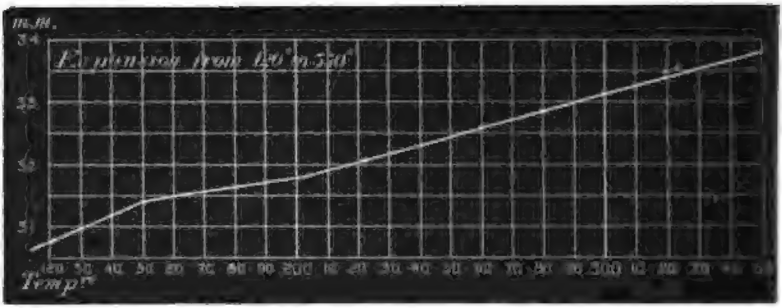
Expansion.

Having failed to obtain measurements with the specific gravity

bottle from the difficulty of preserving the bottle on remelting the sulphur, a tube was used shown in the figure at A. The capacity of the bulb was 13·6 cm. The second vessel contained sufficient mercury to cover the bulb of A, and the stem to the point to which the sulphur rose on heating.

The bulb was filled with sulphur, and the whole of it kept beneath the surface of the mercury except when a reading was taken.

From several series of experiments the curve was prepared.



Chemical Affinity.

If the changes previously noticed are produced by some change in the molecules of the element, it will probably show itself in the action of sulphur when strips of metal are exposed to its attack. We know that some metals are acted upon at ordinary temperatures in a slight degree, and with increased energy as the temperature rises. Others do not appear to be changed until a high point. It therefore appeared probable that by carefully watching strips of different metals exposed to sulphur at various temperatures, it might be discovered whether there was any point of sudden increase, and if so what relation it bore to the curves already obtained.

A test tube was used for the sulphur, and a strip or piece of the metal having been placed in it, the tube was immersed in heated mercury.

Temperature.	
120° C....	After expelling all the air by a stream of coal-gas, sodium was dropped in. Took fire.
180°......	Heated four hours with occasional shaking.
	Hg, Cu, and Pb slowly attacked.
	Mg, Zn, and Sn not.
245 to 270°	Hg formed a dark malleable mass, filled with globules of the metal.
	Cu more readily acted on.

Temperature.	
290 to 310°	Cu almost eaten away.
	Pb as before.
	Mg, Zn, and Sn not attacked.

Again pieces of copper, of equal sizes and weights, were cut from the same sheet. After having been carefully cleaned and weighed they were exposed to sulphur at different temperatures for 15 minutes, after which they were carefully cleaned and re-weighed.

Weight of copper used.	Temperature.	Loss of weight.	Rise of temperature.
(1) 11·34 grams..	240°	0·89	12°
(2) " ..	280	0·92	11
(3) " ..	300	1·62	30

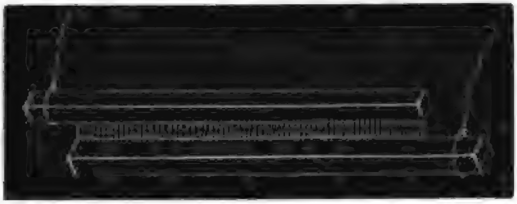
The last column gives the increase of temperature due to the union of the copper and the sulphur. It occurred two minutes after the cold copper was introduced. At first there was a fall of 4° in the two first experiments, and 5° in the last; this was followed by a rise. The lamp was withdrawn when the temperature rose to that at which the experiment commenced. At the end of two minutes the rise given in the fourth line was observed.

It appears, therefore, that there is a gradually increasing action up to 290°, or about that temperature, and above that a considerable increase. The point of change in resistance, 290°, appears to be one of considerable importance, carrying also the fluidity, boiling point, and chemical affinity, &c.

Action of Light on Sulphur.

That the metals of the same group in the arrangement according to the Periodic Law have properties in common is well known. Thus chlorine, bromine, and iodine belong to the same group, and are in many respects very similar bodies. Sulphur belongs not only to the same group as selenium, but is the next element in front of it. Naturally, therefore, we expect that they will have properties in common, and possibly the action of light in the case of selenium may be shared in an inferior degree by sulphur. This appears more probable from the well-known fact that a saturated solution of sulphur in bisulphide of carbon is rendered turbid by direct sunlight, part of the sulphur being changed and becoming insoluble in that liquid. A portion of the sulphur undergoes the same change when exposed to a high temperature. In order that the sulphur used in the experiment might be as sensitive as possible to light, it appeared desirable

that only pure soluble sulphur should be used, and that great care should be taken not to raise the temperature in melting it so high as to produce any of the insoluble modification.



Two rectangular graphite rods were placed parallel to each other, the one projecting about an inch at one end, the other at the other end.

The edges were turned towards each other, as shown in the figure, leaving a space of one millimetre, which was filled with melted sulphur. This was levelled off with a hot iron to make the portion between the corners as thin as possible.

When one of the projecting ends was placed in contact with a charged electroscope, the other being to earth, the charge fell more quickly when illuminated, on the average as 5 is to 4.

As little reliance can be placed on these experiments, a quadrant electrometer was charged, and the graphite rods, separated by sulphur, inserted between the binding screws, so that the negative quadrant was connected with the positive one through the sulphur (1·5 cm. long, 1 mm. thick, and 1 mm. broad).

The electrometer was charged to the same potential in each experiment in a series and allowed to run down for a certain time. Sunlight was allowed to fall on the sulphur, but shaded from the rest of the apparatus; when not required the ordinary window blind was drawn down.

Of course the electrometer and sulphur were protected from induction by surrounding bodies by wire screens.

The following three series of readings were taken on different days, and in one or two cases clouds interfered with the experiment, especially in No. 8, when the light was considerably shaded by cloud.

	Scale deflection at the beginning.	Time.	Fall in scale divisions.	State.
		minutes.		
1st series (1) .	180	30	20	Dark.
(2) .	,,	15	20	Light.
(3) .	,,	35	18	Dark.
2nd series (4).	300	15	25	Light.
(5).	,,	17	25	,,
(6).	,,	15	20	Dark.
(7).	,,	15	19	,,
(8).	,,	15	20	Light.
3rd series (9).	200	15	14	Light.
(10)	,,	15	17	,,
(11)	,,	16·5	16	,,
(12)	,,	15	10	Dark.
(13)	,,	15	10	,,

The first and second series were alternated dark and light in the same set of experiments to see that no permanent change was produced and mistaken for the effect sought. In the first the time varied and the deflections were allowed to fall the same distance; in all the others the time was the same and the fall varied.

The method of performing the experiments made it possible that the effect might be produced by the heat of the sun and not by the light. The variation in temperature observed on a delicate thermometer was about 1° C. To eliminate the effect of heat, a long series of observations was made in the dark, whilst the temperature was raised slightly by placing a Bunsen flame 4 or 5 inches away from the screen protecting the sulphur, and the heat radiated by placing an iron spiral in it, then one of copper, and lastly a fine clay tile.

The range of temperature was 15·2° C. to 17·1° C. in the first ten experiments, in which the heated ones fell rather more slowly than those at a lower temperature. In the next seven observations the range was 14·8° C. to 18° C., the fall being exactly the same in each.

There yet remained the possibility that the light falling on the wires which held the rods caused the charge to escape more quickly into the air. When, however, the sulphur was removed, the effect produced by the light on the portions of wire exposed to its influence was too small to produce any change in the rate of fall.

Hence it appears that although selenium is the body most sensitive to the action of light, it shares its property with its neighbours, and the three elements (constituting the same group), sulphur, selenium, and tellurium, are all similarly acted upon, furnishing another example

of the importance and beauty of the law which classified them together.

During the course of this work I have often consulted Professor J. J. Thomson, F.R.S., and received many valuable suggestions and some corrections, for which I desire to acknowledge my obligation.

Addendum. May 22, 1889.

It having been suggested that the passage of the current at high temperatures through roll and precipitated sulphur was caused by the presence of impurities, and not by any change in the properties of the sulphur itself, that some of the impurities distil over with the element, especially sulphuric acid, compounds of mercury and selenium, whose presence would be quite sufficient to account for the effects given by the specimens used in the previous experiments, it was necessary to obtain the purest possible specimens of sulphur, and with this object no pains have been spared.

As there also arose the question whether a liquid, being neither a metal nor an electrolyte, could conduct an electric current, it appeared to be preferable to try various methods of purification, and to compare the results obtained.

To make this comparison more valuable, the methods should not be simply variations of the one system, but proceed upon distinct lines, so that any impurities, left after all possible care had been used, should be different in the different specimens, and in the measurements there would appear the effects due to distinct bodies, and if so, give some indication of the presence, in one or other of the portions used, of some foreign body changing the electric properties of the melted sulphur differently from the cases in which that particular body could not possibly occur. Three methods of purification were employed:—1st, solution, crystallisation, and distillation; 2nd, distillation, without solution, in an atmosphere intended to remove hydrogen compounds; 3rd (pure soluble bodies only were used, easily tested chemically), precipitation, washing with water, and distillation.

In numbers 1 and 2, foreign bodies acting upon sulphur were added and afterwards removed; they were different bodies, and if not perfectly removed might be expected to change the conductivity according to their own individual properties. In number 3 no such body was introduced. Consequently, if 1, 2, and 3 were alike in their resistances at various temperatures, it must arise from changes in the one body common to all three, namely, sulphur.

Before giving further particulars of these three methods, I wish to describe experiments undertaken to prevent the electrodes and the containing vessels from spoiling the liquid after it had been purified.

In the course of the work it was found that the vessels in which sulphur was boiled for any length of time were attacked. Ordinary test-tubes invariably gave way, becoming coated internally with a thin black film, which remained fixed to the glass.

Ordinary tubes and glazed porcelain under the same circumstances showed a number of dark spots, which proved to be sulphide of iron; even combustion-tubing did the same thing.

All these experiments had been made with the flame of a bunsen lamp acting directly upon the vessel containing the sulphur, and it appears that no material will resist the attack of that body under such conditions. It seems to be caused by over-heating a portion of the vessel from which the liquid has been separated by the vapour, when bubbles are formed. When the liquid falls back upon this overheated surface, chemical action commences.

Further experiment, however, showed that when combustion-tubing, or retorts of Bohemian glass, are protected from the direct action of the flame, sulphur may be boiled or distilled in them without any action whatever taking place.

Investigations were also made on the nature of the electrodes, by means of which a current could be made to pass through sulphur, and upon the best form to be used to avoid chemical action. In the previous work I used ordinary thin platinum wire, and failed to get a current to pass.

In repeating this experiment I used a wire of one millimetre diameter, which had been exposed to great heat for a considerable length of time, and found that the current passed readily.

The same thing took place when some thin, very hard, carbon rods were substituted for the platinum. The liquid was, however, dirtied by particles of carbon torn away from the rods, and consequently I have not considered the numbers obtained worthy of being recorded, but simply the fact that with hard carbon rods for electrodes, sulphur will conduct; probably electric lamp filaments may prove good enough to resist disintegration, if not too thin for the purpose.

To test the effect of sulphur upon the electrodes, the containing vessel was placed in a second one, half filled with mercury, which was kept boiling several hours; this produced a steady temperature of 350°. The platinum electrodes were immersed in the liquid, and after allowing them to remain undisturbed until the whole mass had assumed the proper temperature, measurements were taken at stated intervals. The method was to use a steady electromotive force, with the sulphur in circuit, and a high resistance galvanometer. The numbers given below show a conductivity increasing with the time of contact between the electrodes and the hot sulphur. After the experiment I found that the platinum was considerably discoloured.

Table showing the action of Heated Sulphur upon Platinum Electrodes.

After an exposure of—		Deflection produced by a constant electromotive force.
60 minutes	......	22 scale divisions.
90 "	......	29 "
120 "	......	32 "
140 "	......	33 "
200 "	......	40 "
260 "	......	43 "
275 "	......	44 "

Lastly, the same graphite rods were used as in the previous experiments. These electrodes had been repeatedly exposed to high temperatures, and also boiled many times in sulphur. After being kept at temperatures varying probably not more than from 400° C. to 440° C. during five hours, the conducting power of the liquid was practically the same as at first.

Thus, at 12.50 P.M. (boiling) the deflection was 290 scale divisions. The flame was then slightly reduced, and at—

1.20 P.M.........	260 scale divisions.
3.30 P.M.........	150 "

The flame was then raised again, and at—

4.30 P.M.........	220 scale divisions.
5.30 P.M. (boiling)	280 "
5.50 P.M. "	280 "

New graphite electrodes were next tried, and found to discolour the sulphur considerably; but the resistance was increased. In all cases the current passed. The objection to using the same electrodes, even after using great care in cleaning them in the different liquids, and in that way contaminating them, and the impossibility of getting new ones that would do without previous boiling in sulphur, caused me to abandon their use altogether, and to depend upon a short exposure of platinum instead. The conclusion, determined by the work described, was that using combustion-tubing for boiling and well-cooked graphite electrodes, the change produced by chemical action is practically nothing, even after boiling several hours, if the containing vessel be protected from the direct flame, and that in the case of platinum electrodes, it tne observations are taken immediately they are inserted into the liquid, the action is slow enough to allow measurements to be taken without fear of error.

Purification of the Sulphur.

In the first method, for which I am indebted to Dr. Ruhemann, of the Chemical Laboratory, Cambridge, bisulphide of carbon was purified by being shaken with a little mercury and allowed to stand. It was afterwards distilled over dry calcium chloride. These operations were repeated until the liquid was separated from other sulphides and from water. It was then saturated with sulphur and half of the liquid distilled off. On cooling, crystals of sulphur formed. These were removed and washed with fresh bisulphide to remove any impurities that might have been left on their surfaces by the evaporation of the adhering mother-liquid. They were then carefully broken up and placed in a vacuum to remove as much of the bisulphide as possible before distilling. After remaining so for several days, they were distilled *in vacuo* several times, the first portion coming over, and that portion remaining behind being rejected in each case.

The substance thus produced was of a beautiful light yellow colour, and melted into a perfectly clear, transparent liquid, about the colour of olive oil; at higher temperatures it assumed the tint of port wine. No traces of sulphuric acid, nor of chlorides, could be found, and the absence of selenium was proved in the original substance.

The only objection to this method of working is the presence of a body whose solvent power for sulphur is so great, and the possibility that the last traces are not removed even by repeated distillations *in vacuo*.

The second method consisted in distilling precipitated sulphur in an atmosphere of chloride of sulphur, which removes hydrogen compounds. After repeating this several times, it was distilled *in vacuo*. As before, the middle portion only was retained. This method has been found to give good results in the hands of some experimenters, but I found very great difficulty in removing the chloride, being obliged to reduce the body to a fine powder, and wash with water, and finding this insufficient, finally distilled over a few small pieces of pure zinc *in vacuo*, after which it was redistilled.

This specimen was distilled altogether eleven times.

The third method is the one used by Professor Threlfall, who takes hyposulphite of soda, free from selenium, and dissolves it in distilled water, then precipitates the sulphur by means of pure hydrochloric acid.

The reaction is shown by the well-known equation—

$$Na_2S_2O_3 + 2HCl = 2NaCl + SO_2 + H_2O + S.$$

All the substances produced, being either soluble in water, or gas evolved during the reaction, except the sulphur itself, can be washed out with pure water.

To avoid the addition of any objectionable body, no attempt was made to precipitate the sulphur from the sulphur dioxide, hence half the sulphur present was lost.

Thus it will be seen at once that a considerable quantity of the salt was required to produce a very small quantity of pure sulphur. 14 lbs. of the hyposulphite were dissolved, filtered, and decomposed by acid, then washed until free from salts and acids, dried, and distilled several times. When the residue appeared to be perfectly free from foreign matter it was repeatedly distilled *in vacuo*, the middle portion being removed.

In this method the only solvent was water, and the other bodies produced could be tested for by delicate chemical reactions. I believe that the three methods described fulfil the requirements mentioned in an earlier portion of this paper, giving as pure sulphur as can be prepared, but at the same time, the bodies that may have escaped removal will differ in each specimen.

An improvement was introduced into the insulation of the electrodes. The platinum wires were fused into glass tubes from 12 to 14 inches in length. These were fixed into hard dry wood at a distance of several inches from their ends. In this way the only part that can possibly conduct is removed further from the source of heat.

The ends of the electrodes were flat plates, formed by bending the platinum wire upon itself three or four times, and then welding it together, also for greater security a strip of platinum-foil was welded to the back of each.

The length of AB was 3·35 cm., the width 0·45 cm., and the distance apart 0·2 cm.

The tube in which the sulphur was boiled was formed by fusing up the end of a piece of combustion-tubing about 9 inches long, and wide enough to allow the glass tubes to be inserted in the sulphur without danger of touching the sides, and thus forming a circuit through hot glass.

It was surrounded to the height of 3 inches by a copper tube, closed at the bottom, the intervening space being filled with sand.

To avoid the chemical action which has been shown to take place when platinum is exposed for any length of time to sulphur at a high temperature, the electrodes were kept out of the liquid until everything was ready for taking a reading; they were then inserted, and the readings having been taken as quickly as possible, they were removed.

At the end of each set of experiments the wires were examined and found in every case to be free from any appearance of the dark film observed in the preliminary work.

Before introducing them into the next specimen of sulphur they were ignited in the blowpipe-flame until perfectly clean.

The method used was to place the sulphur in circuit with a battery and a high resistance galvanometer (R 11,700 ohms). In order to avoid chemical action it was considered better to reduce the number of observations and to commence with the boiling point.

At 440° C.	pure sulphur	gave a	deflection of	545 to 570	divisions.
350° C.	,,	,,	,,	75	,,
300° C.	,,	,,	,,	15	,,

Those previously found, for precipitated sulphur at the same temperature, were 512, 73, and 15. I have therefore concluded that the two curves are identical.

When the sulphur was removed from the circuit and a known resistance inserted, the calculated specific resistance was about one-fifth larger than that given by precipitated sulphur.

I have calculated the specific resistance from the experiments, more as an indication of the magnitude of the resistance at the boiling point, and of the changes that take place as the temperature varies, than as an accurate determination of specific resistance.

There are several circumstances which prevent the great accuracy usually expected in such cases. First the extreme difficulty of obtaining a steady temperature without exposing the electrodes to chemical action. Thus if the readings be taken at 350° C., by using a bath of boiling mercury it requires a considerable time to get the whole mass of sulphur to this temperature, the sulphur being a very bad conductor of heat and there being no agitation to assist. When the whole is steady and the electrodes are introduced, they cool the portion in contact with them, and it is necessary to wait until the temperature rises again. Hence arises an uncertainty, we may take it before the temperature is fully recovered, or we may delay too long and allow chemical action to commence. The same objection applies, in some measure, to boiling sulphur, but as the whole is in motion the recovery is quick.

Great care is necessary to prevent bubbles of gas rising up between the electrodes and so increasing the resistance.

Some error might also arise from the size of the electrodes, 3·35 cm. × 0·45 cm., distance 0·2 cm. They were as large as the quantities of pure sulphur obtained by nearly three months' work enabled me to use them.

With these reservations I give the specific resistance of melted sulphur, calculated from experiments with the three specimens mentioned.

Specific Resistances.

No. 1.	At 440° C.......	7·8	megohms.
2.	„	8·0	„
3.	„	7·3	„
2 & 3.	At 350° C.......	56·5	„
„	300° C.......	282·5	„

Boiling Point.

An objection has been raised to the curve found in the experiment on the boiling point of sulphur under varying pressures, on the ground that the vapour-pressure rises in a straight line, and that, therefore, the boiling point would give a straight line also.

It is usually stated in text-books that when the vapour-pressure of a liquid becomes equal to the pressure on the surface of that liquid it immediately begins to boil. If this is a scientific fact, the objec-

tion urged is good, and the vapour-pressure of any liquid and the boiling point of that liquid must always be on the same line. I think, however, that it is never absolutely correct, and sometimes it is very far from being true.

If we suppose a thin film of the liquid to be acted upon with a downward pressure produced by the air and an upward force produced by its vapour-pressure, these two forces, by hypothesis, equal and opposite in direction, can produce no motion in the film. Let the force necessary to produce this motion be called (a).

Besides the mere upward motion of the film in a bubble, there is an expanding action which draws out the substance of the bubble, and is resisted by it with force depending upon the nature of the liquid. Let this be called (b).

Finally, there is a certain amount of force required to burst the particles of the liquid apart when the bubble begins to form. Let this be (c).

We have, therefore, when bubbles are formed in any liquid, a force equal to the pressure on the surface of the liquid together with $a + b + c$.

If the viscosity of the liquid remains the same through the whole range of temperature, $a + b + c$ will remain the same, and the line for vapour-pressure and that for boiling point will be parallel, but if instead of this, the liquid changes from being a liquid as mobile as water to a thick viscous body, so stiff that the vessel containing it may be inverted without one drop of the substance being lost, $a + b + c$ will change also, and the two lines will not be parallel. The forces $a + b + c$ will be a function of the viscosity.

To test the truth of this reasoning, I carefully repeated the experiments, and found the results to agree with those previously obtained. At the same time, besides noting the pressures at which the bubbles began to form on the surface of the liquid at various temperatures, I observed the pressures at which these bubbles burst, and found that there was a considerable difference. Up to 280° C. it was 4 mm. of mercury, while at 296° C. it fell to 1 mm.,

or $\quad$ A+B = 4 mm. of mercury up to 280° C.
$\quad$ A+B = 1 ,, ,, 296° C.

I did not attempt to measure the force C, but I think it probable that it is much greater than A+B, and that the variation of these (A+B+C) in sulphur explains the form of the line found.

The sulphur molecule is known to undergo various changes, at one temperature containing six atoms, while at another only two enter into its formation. What are the molecular modifications that take place when it cools to a liquid, or when it assumes a semi-fluid state

and at last turns back again to liquid, we do not know. But when one of these changes is accompanied by a corresponding one in chemical activity, it appears to mark a point at which the complex molecules are being broken into others of less complex structure.

As this is the temperature at which the conductivity changes, I am inclined to suspect that the current is carried by the simpler molecules, as they break apart and recombine, acting, to a certain extent, the part of the different elements in an ordinary electrolyte. Supposing this to be the solution of the question, other elements that undergo similar molecular changes should give indications of a like nature, and I am at present engaged in work with the object of seeing if it is so.

Sold by Harrison and Sons, St. Martin's Lane.

PHILOSOPHICAL TRANSACTIONS.

Price £1 10*s*., with 23 Plates.

SERIES A. Containing Papers of a Mathematical or Physical Character.

VOL. CLXXIX (1888).

Price £2 17*s*. 6*d*., with 80 Plates.

SERIES B. Containing Papers of a Biological Character.

VOL. CLXXIX (1888).

Cloth cases for binding the above volumes can be obtained of Harrison and Sons, St. Martin's Lane. Price 1*s*. 6*d*.

The Papers in each Series can also be purchased separately and are published by Trübner & Co., 57, Ludgate Hill. The following are now ready:—

SERIES A.

VOL. CLXXX (1889).

39. Total Eclipse of the Sun observed at Caroline Island, on 6th May, 1883. By Captain W. DE W. ABNEY, C.B., R.E., F.R.S. Price 5*s*. 6*d*.

40. On Evaporation and Dissociation. Part VIII.—A Study of the Thermal Properties of Propyl Alcohol. By Professor WILLIAM RAMSAY, Ph.D., F.R.S., and Professor SYDNEY YOUNG, D.Sc. Price 3*s*.

41. The Radio-micrometer. By C. V. BOYS, Assoc. Royal School of Mines, Demonstrator of Physics at the Science Schools, South Kensington. Price 1*s*.

42. The Waves on a Rotating Liquid Spheroid of Finite Ellipticity. By G. H. BRYAN, B.A. Price 1*s*. 6*d*.

43. On the Magnetisation of Iron and other Magnetic Metals in very strong Fields. By J. A. EWING, B.Sc., F.R.S., Professor of Engineering in University College, Dundee, and WILLIAM LOW. Price 1*s*. 6*d*.

44. Some Observations on the Amount of Light reflected and transmitted by certain kinds of Glass. By Sir JOHN CONROY, Bart., M.A., Bedford Lecturer of Balliol College, and Millard Lecturer of Trinity College, Oxford. Price 2*s*. 6*d*.

SERIES B.

VOL. CLXXX (1889).

41. On the present Position of the Question of the Sources of the Nitrogen of Vegetation, with some new Results, and preliminary Notice of new Lines of Investigation. By Sir J. B. LAWES, Bart., LL.D., F.R.S., and Professor J. H. GILBERT, LL.D., F.R.S. Price 4*s*. 6*d*.

42. On the Secretion of Saliva, chiefly on the Secretion of Salts in it. By J. N. LANGLEY, M.A., F.R.S., Fellow of Trinity College, and H. M. FLETCHER, B.A., Trinity College, Cambridge. Price 2*s*.

43. On the Organisation of the Fossil Plants of the Coal-measures. Part XV. By WILLIAM CRAWFORD WILLIAMSON, LL.D., F.R.S., Professor of Botany in the Owens College, Manchester. Price 2*s*. 6*d*.

44. On the Electromotive Changes connected with the Beat of the Mammalian Heart, and of the Human Heart in particular. By AUGUSTUS D. WALLER, M.D. Price 2*s*.

Published by Trübner and Co.

Royal 4to. pp. xiv–326, cloth. Price 21*s*.

OBSERVATIONS OF THE INTERNATIONAL POLAR EXPEDITIONS.

1882–1883.

FORT RAE.

With 32 Lithographic Folding Plates.

A reduction of price to Fellows of the Royal Society (see above).

Sold by Harrison and Sons.

CATALOGUE OF THE SCIENTIFIC BOOKS IN THE LIBRARY OF THE ROYAL SOCIETY.

FIRST SECTION:—Containing Transactions, Journals, Observations and Reports, Surveys, Museums. Price 5*s*.

SECOND SECTION:—General Science. Price 15*s*.

A Reduction of Price to Fellows of the Society (see 4th page of wrapper).

Sold by Harrison and Sons.

CATALOGUE OF SCIENTIFIC PAPERS,

Compiled by the Royal Society.

Vols. 1 to 8. Price, each volume, half morocco, 28*s*., cloth, 20*s*.

A reduction of one-third on a single copy to Fellows of the Royal Society.

HARRISON AND SONS, 45 & 46, ST. MARTIN'S LANE, W.C.,

AND ALL BOOKSELLERS.

tion urged is good, and the vapour-pressure of any liquid and the boiling point of that liquid must always be on the same line. I think, however, that it is never absolutely correct, and sometimes it is very far from being true.

If we suppose a thin film of the liquid to be acted upon with a downward pressure produced by the air and an upward force produced by its vapour-pressure, these two forces, by hypothesis, equal and opposite in direction, can produce no motion in the film. Let the force necessary to produce this motion be called (a).

Besides the mere upward motion of the film in a bubble, there is an expanding action which draws out the substance of the bubble, and is resisted by it with force depending upon the nature of the liquid. Let this be called (b).

Finally, there is a certain amount of force required to burst the particles of the liquid apart when the bubble begins to form. Let this be (c).

We have, therefore, when bubbles are formed in any liquid, a force equal to the pressure on the surface of the liquid together with $a + b + c$.

If the viscosity of the liquid remains the same through the whole range of temperature, $a + b + c$ will remain the same, and the line for vapour-pressure and that for boiling point will be parallel, but if instead of this, the liquid changes from being a liquid as mobile as water to a thick viscous body, so stiff that the vessel containing it may be inverted without one drop of the substance being lost, $a + b + c$ will change also, and the two lines will not be parallel. The forces $a + b + c$ will be a function of the viscosity.

To test the truth of this reasoning, I carefully repeated the experiments, and found the results to agree with those previously obtained. At the same time, besides noting the pressures at which the bubbles began to form on the surface of the liquid at various temperatures, I observed the pressures at which these bubbles burst, and found that there was a considerable difference. Up to 280° C. it was 4 mm. of mercury, while at 296° C. it fell to 1 mm.,

or $\quad$ A+B = 4 mm. of mercury up to 280° C.
$\quad\quad$ A+B = 1 „ „ 296° C.

I did not attempt to measure the force C, but I think it probable that it is much greater than A+B, and that the variation of these (A+B+C) in sulphur explains the form of the line found.

The sulphur molecule is known to undergo various changes, at one temperature containing six atoms, while at another only two enter into its formation. What are the molecular modifications that take place when it cools to a liquid, or when it assumes a semi-fluid state

and at last turns back again to liquid, we do not know. But when one of these changes is accompanied by a corresponding one in chemical activity, it appears to mark a point at which the complex molecules are being broken into others of less complex structure.

As this is the temperature at which the conductivity changes, I am inclined to suspect that the current is carried by the simpler molecules, as they break apart and recombine, acting, to a certain extent, the part of the different elements in an ordinary electrolyte. Supposing this to be the solution of the question, other elements that undergo similar molecular changes should give indications of a like nature, and I am at present engaged in work with the object of seeing if it is so.

May 23, 1889.

Professor G. G. STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The Croonian Lecture was delivered as follows:—

CROONIAN LECTURE.—"Les Inoculations Préventives." By Dr. E. ROUX, Institut Pasteur, Paris. Delivered May 23,—MS. received May 23, 1889.

MESSIEURS,

Au mois d'Août, 1881, M. Pasteur faisait connaître aux membres du Congrès Médical International, réuni à Londres, les récents travaux de son laboratoire sur les inoculations préventives du choléra des poules et du charbon. Huit années sont presqu'écoulées depuis cette époque. Qu'est devenue l'œuvre commencée alors, a-t-elle justifié les espérances qu'elle faisait naître? Quelle place ont pris dans la science les principes nouveaux qui venaient d'y être introduits? C'est ce que M. Pasteur devait exposer devant vous aujourd'hui. Mais l'état de sa santé ne lui a pas permis de répondre à l'honneur que lui avaient fait le Président et le Conseil de la Société Royale, en le conviant à faire la lecture de cette année. Il a proposé au Président et au Conseil de votre Société d'accepter que je parle en son nom. Je ne saurais, Messieurs, vous parler, comme l'aurait fait M. Pasteur, de ces inoculations préventives qu'il a inventées, et je crains bien d'augmenter aujourd'hui, par mon discours, le regret que vous cause déjà son absence.

Je n'ai qu'un titre qui puisse expliquer que je sois à cette place,

c'est celui de collaborateur de M. Pasteur. J'ai eu, en effet, avec MM. Chamberland et Thuillier, l'honneur d'être associé aux travaux sur la prévention des maladies contagieuses, et mon excuse pour oser prendre la parole devant vous est que j'ai vu les choses dont je vais parler.

Beaucoup de maladies infectieuses ne récidivent pas. Le plus souvent on n'a qu'une fois la variole, la rougeole, la fièvre typhoïde etc. Une première atteinte, même légère, met à l'abri de ces maladies pour un temps plus ou moins long. Cette observation de la non-récidive des maladies infectieuses a conduit à l'inoculation préventive. Au lieu d'attendre d'être frappé à l'improviste par la maladie, souvent pendant une épidémie très meurtrière, dans des conditions défavorables à la résistance, on a cherché à prendre cette maladie à un moment choisi, avec toutes les précautions capables d'en diminuer le danger. A la contagion naturelle, imprévue, et sur laquelle on n'a pas d'action, on a substitué la contagion artificielle, préparée de façon à donner l'immunité avec le moins de risques possibles. C'est contre la variole que l'on a eu recours pour la première fois à l'inoculation préventive. En effet, une expérience involontaire et trop souvent renouvelée, avait appris que le liquide de la pustule variolique est virulent, c'est-à-dire que cette lymphe varioleuse introduite dans le corps par une blessure de la peau, communique la maladie à une personne qui ne l'a pas eue encore. L'inoculation de la variole était donc facile. Il suffisait pour la réaliser de la piqûre d'une lancette chargée de pus varioleux. Dans la pratique, on recherchait un cas de variole bénigne et dans les pustules on puisait un virus, supposé peu actif, mais capable de conférer l'immunité à ceux qui le recevaient. Vous savez, Messieurs, qu'elle extension prit la variolisation, qui était loin cependant d'être inoffensive, puisque l'inoculation que l'on croyait devoir donner une maladie légère procurait souvent une maladie grave, parfois même mortelle.

Aussi, combien grand a été le progrès dû à Jenner qui a remplacé la variolisation par la vaccination. A l'inoculation d'une maladie grave, Jenner substituait celle d'une maladie toujours inoffensive et qui met efficacement à l'abri de la variole.

Depuis le commencement de ce siècle nous jouissons de l'inappréciable bienfait de la vaccination Jennérienne, et cependant nous n'en avons pas encore pénétré le secret. Quelle relation y a-t-il entre la vaccine et la variole? Pourquoi la vaccine, maladie du cheval et de la vache, inoculée à l'homme, le préserve-t-elle de la variole? Le virus vaccin est-il le virus modifié de la variole, ou bien variole et vaccine sont-elles deux maladies différentes? Il semble que ces questions soient faciles à résoudre, puisque l'on peut expérimenter sur la variole et la vaccine. Posées depuis Jenner elles sont encore sans réponses précises. La grande découverte Jennérienne, si bien faite

pour éveiller les espérances, est restée unique en médecine. Née d'une observation heureuse, merveilleusement développée par un génie aussi patient que pénétrant, elle était, à l'époque où elle a été faite, si en avance sur la médecine, qu'aujourd'hui après tant de progrès, nous ne pouvons qu'en soupçonner la véritable interprétation. Jenner nous a montré, par un extraordinaire exemple, que l'on peut préserver d'une maladie mortelle par l'inoculation d'une maladie bénigne, mais il ne nous a pas donné de méthode pour nous conduire à la prévention des autres maladies infectieuses. La découverte de l'atténuation artificielle des virus nous fournit, au contraire, une véritable méthode d'inoculation préventive qui a déjà donné une suite ininterrompue de résultats heureux, bien qu'elle date à peine de quelques années. Comme tous les autres progrès accomplis récemment dans la connaissance des maladies virulentes elle a pour origine les recherches de M. Pasteur sur les fermentations.

En nous dévoilant la nature des ferments, M. Pasteur nous a appris celle des virus. Comme la levure alcoolique et la levure lactique, les virus sont des êtres vivants, des microbes comme on dit aujourd'hui. De même que le développement de la levure, dans un liquide sucré, produit la fermentation alcoolique, de même celui des microbes dans le corps produit la maladie infectieuse.

Les procédés qui ont réussi pour obtenir la culture des microbes-ferments à l'état de pureté sont ceux qui ont permis la culture pure des microbes-virus en dehors de l'organisme. La condition expresse pour réussir ces cultures, c'est d'agir avec pureté, c'est-à-dire, d'éviter l'introduction des germes étrangers qui sont partout autour de nous. Une technique bien établie maintenant, rigoureuse en même temps que très simple, permet d'obtenir ce résultat.

Puisque les virus sont des êtres vivants que l'on peut entretenir en cultures artificielles, et qu'ils ne se distinguent des autres plantes microscopiques que par la propriété qu'ils ont d'envahir le corps de l'homme et des animaux, ne serait-il pas possible de les modifier par la culture comme on modifie les autres plantes? Ne pourrait-on pas par exemple les priver des qualités qui les rendent redoutables? Modifier les virus par des conditions de culture spéciales, telle est l'idée de M. Pasteur, idée féconde d'où est sortie la suite de découvertes que je vais vous exposer.

C'est en étudiant une maladie des volailles appelée "choléra des poules," que M. Pasteur a obtenu pour la première fois un virus atténué. Cette maladie est si meurtrière pour les lapins, les poules, les pigeons et les oiseaux en général, qu'on lui a donné le nom de choléra. Elle est causée par le développement, dans le corps des animaux qui en sont frappés, d'un microbe très petit, en forme de bâtonnet, à bouts arrondis, presque aussi large que long. La photographie projetée sur l'écran nous montre l'image au microscope d'une gouttelette du sang

d'une poule qui a succombé à la maladie spontanée. Vous voyez, entre les globules du sang, les petits bâtonnets qui sont la cause de la maladie. Il n'y a pas que le sang qui contienne le microbe; tous les tissus sont envahis par lui. Les intestins en renferment une grande quantité, de sorte que les déjections des poules malades peuvent répandre la maladie. C'est en picorant sur le sol souillé que les volailles saines sont contaminées.

Si l'on introduit, sous la peau d'une poule en bonne santé, une trace du sang d'une poule qui vient de succomber au choléra spontané, l'animal inoculé tombe bientôt malade; il ne mange plus; il tient ses plumes hérissées, ses ailes pendantes, et il semble accablé par une somnolence invincible. La mort survient souvent en moins de douze heures. Le sang de la poule qui a succombé à l'inoculation expérimentale est envahi par le microbe, absolument comme le sang des volailles qui meurent à la suite de la contagion naturelle.

Le choléra des poules nous apparaît donc comme une maladie contagieuse, inoculable, dont le virus est contenu dans le sang des animaux qui en sont frappés.

La culture du microbe qui se fait si facilement dans le sang des animaux peut aussi être obtenue en dehors de l'organisme. Au moyen d'un fil de platine, d'abord chauffé au rouge puis introduit dans le cœur ou dans un vaisseau d'une poule morte du choléra, portons une trace de sang dans un flacon comme celui-ci, qui contient un peu de bouillon de poule légèrement alcalin et parfaitement limpide. Plaçons ensuite ce flacon dans une étuve à 35°. Au bout de quelques heures le bouillon est troublé, et le trouble est dû au développement du petit microbe du choléra des poules. Au microscope chaque gouttelette du bouillon nous montre une quantité innombrable de petits bâtonnets immobiles, semblables à ceux qui étaient contenus dans le sang qui a servi de semence. Une quantité infiniment petite de cette première culture, déposée dans un nouveau flacon de bouillon, donnera une seconde culture. Par des ensemencements successifs, on pourra produire des générations de notre microbe aussi nombreuses qu'on le voudra. Chaque goutte de ces cultures, de la vingtième aussi bien que de la première, tuera avec tous les signes du choléra la poule à laquelle on l'inoculera. Cette expérience nous donne la preuve décisive que le virus de la maladie est bien le microbe contenu dans nos cultures, et puisque nous savons préparer *in vitro*, dans des conditions bien précises, des quantités de virus du choléra aussi grandes que nous le désirons, nous sommes vraiment outillés pour l'étude de cette maladie.

Laissons à 35°, au contact de l'air pur filtré à travers le tampon de coton qui ferme le flacon, une de ces cultures, si active qu'elle tue toutes les poules auxquelles on l'inocule. Chaque semaine, prélevons un peu du contenu de ce flacon, et essayons sa virulence sur des poules en bonne santé. Pendant les premières semaines de

l'expérience, toutes les poules inoculées succombent ; mais, après un temps plus long, un changement paraît survenir dans la virulence ; toutes les poules ne meurent plus quand on leur injecte sous la peau cette culture plus ancienne. Quelques-unes se rétablissent après avoir été très malades. A mesure que le temps s'écoule, l'activité du virus diminue et le nombre des volailles qui résistent après l'inoculation devient de plus en plus grand. En continuant l'expérience, il arrivera un moment, après deux mois de séjour à l'étuve par exemple, où notre virus, d'abord si meurtrier, non seulement ne tuera plus aucune des poules inoculées, mais encore ne leur causera aucun malaise apparent. Et cependant, le virus n'est pas mort, puisqu'il pullule dans le milieu nutritif où on le sème. Dans cette culture nouvelle, il ne reprend aucune virulence. Les cultures filles ont sur les poules exactement l'action qu'avait la culture mère au moment où celle-ci a fourni la semence. Les propriétés nouvelles du virus, celle d'être devenu inoffensif pour les animaux qu'il tuait tout d'abord, peuvent donc se perpétuer dans des générations successives. En faisant des ensemencements de la culture mère à des dates convenables, on obtiendra toute une série de virus d'activité décroissante, capables de donner aux animaux, soit une maladie mortelle, soit une maladie grave, soit une maladie sérieuse, soit une maladie inoffensive.

A quelle influence est due cette diminution graduelle de la virulence ? A l'action continue de l'oxygène de l'air. En effet, si au lieu de faire la culture dans un flacon où le renouvellement de l'air est possible, on la fait dans un tube clos ne contenant que peu d'air, le microbe aura bientôt consommé tout l'oxygène de cet espace restreint, et il cessera de croître, car l'oxygène est nécessaire à son développement. Dans ce tube privé d'air, il ne pullule pas, mais il se conserve vivant pendant un temps très long, ainsi que l'on peut s'en assurer en ensemençant dans du bouillon aéré le dépôt qui se forme dans ces tubes scellés. Après un an de conservation dans ces tubes, le microbe donne des cultures ; celles-ci sont aussi actives qu'une culture récente préparée avec du sang d'une poule morte du choléra spontané. Donc, l'atténuation de la virulence qui se fait dans les cultures au contact de l'air, ne se produit plus à l'abri de l'air.

Messieurs, que de résultats conquis, que de notions nouvelles acquises dans cette seule expérience sur la culture du microbe du choléra des poules ! Par elle, M. Pasteur nous montre que les virus ne sont pas les entités immuables auxquelles on croyait autrefois. Il nous apprend que les virus, comme tous les êtres vivants, peuvent subir des modifications que l'hérédité perpétue, que la qualité virulente se modifie, et que cette modification peut être obtenue artificiellement et ménagée au gré de l'expérimentateur. Par une expérience précise, M. Pasteur nous fait connaître l'action atténuante de l'air, et du même coup il nous explique comment dans la nature se conserve ou

s'épuise l'activité des virus; il nous montre comment une même maladie peut être tantôt bénigne, tantôt maligne.

Nous avons vu que les poules inoculées avec un microbe atténué du choléra convenablement choisi, prenaient la maladie bénigne et se guérissaient bientôt. Inoculons ces poules rétablies avec du sang d'une poule morte du choléra spontané en même temps qu'un nombre égal de poules neuves. Toutes les poules neuves mourront; celles qui ont reçu d'abord le virus atténué résisteront. Elles ont une maladie passagère qui se dissipe rapidement. L'inoculation du virus atténué a été pour elles préventive de la maladio mortelle; elle leur a donné l'immunité. Et si nous faisons aux mêmes animaux plusieurs inoculations successives de virus atténués d'activité croissante, nous les rendrons réfractaires au choléra à ce point, qu'on pourra leur injecter le sang le plus virulent, les placer dans les conditions où elles seront exposées à la contagion naturelle la plus intense, sans qu'elles éprouvent aucun malaise; il sera devenu impossible de les faire périr de cette maladie naguère si redoutable pour elles.

Le virus atténué découvert par M. Pasteur est donc aussi efficace contre le choléra des poules que le vaccin Jennérien contre la variole. Mais tandis que nous ignorons les rapports qui existent entre la variole et le vaccin, rien ne nous est inconnu des relations qui rattachent le virus vaccin du choléra au virus virulent. Ce procédé d'atténuation ne réussit pas seulement dans le cas particulier du choléra des poules, mais il constitue une véritable méthode d'atténuation des virus, méthode qui a affirmé sa puissance en nous donnant le vaccin d'une autre maladie plus intéressante que le choléra des poules, parce qu'elle est un fléau pour le bétail et qu'elle peut se transmettre à l'homme. Je veux parler du charbon.

Mais dans le cas du charbon, il s'est présenté une difficulté qui n'existait pas pour le choléra des poules.

Le virus charbonneux se trouve dans le sang des animaux qui viennent de succomber à cette maladie. Ensemencé dans du bouillon de veau légèrement alcalin, il donne à la température de 30° une culture qui a l'aspect de flocons cotonneux nageant dans un liquide clair. Ces flocons sont formés de longs filaments enchevêtrés, ainsi que le représente la photographie projetée sous vos yeux. Dans l'intérieur de ces filaments, après quelques heures déjà, on voit apparaître des grains réfringents dont le contour devient de plus en plus net. Ces grains brillants sont les germes ou spores du charbon découverts par M. Koch.* Ces spores sont les véritables graines du microbe, et de même que les grains du blé, par exemple, résistent mieux à la chaleur et à la sécheresse que le blé en herbe, de même la

* Le premier exemple de la formation des spores dans les bacilles a été fourni par M. Pasteur dans ses études sur la flacherie des vers à soie. Plus tard M. Cohn a décrit la formation des spores du *Bacillus subtilis*.

spore du charbon supporte sans périr une température de 90°, et l'action d'une foule d'agents qui tueraient la bactéridie en filaments. La spore est donc la forme de résistance du microbe charbonneux, et chaque fois qu'elle trouvera des conditions favorables à sa germination, soit dans le corps d'un animal, soit dans un milieu nutritif artificiel, elle donnera des filaments, et ceux-ci fourniront à leur tour de nouveaux germes. Aussi, si nous abandonnons à la température de 35° et au contact de l'air, comme nous l'avons fait avec le choléra des poules, une culture de charbon, la virulence de cette culture ne diminue pas. Même au bout d'un temps très long elle tue tous les animaux auxquels on l'inocule. L'oxygène de l'air nous paraît ici ne pas exercer d'influence atténuante, parce que les germes, qui sont formés dès les premières heures de la culture, résistent à son action. Il faut donc, pour nous placer dans des conditions analogues à celles qui ont réussi pour le choléra des poules, empêcher la bactéridie de produire des spores. Le moyen pour y arriver consiste à faire la culture du charbon non plus à 35° mais à 42—43°. Dans ces conditions la bactéridie se développe mais ne donne plus de graines, elle reste donc exposée, à l'état de filaments, à l'action continue de l'air et de la chaleur.

Essayons, tous les trois jours par exemple, en l'inoculant à des moutons et à des lapins, la virulence d'une culture faite ainsi à haute température et dans laquelle les spores ne se forment pas. Dans les premiers jours de l'expérience tous les animaux inoculés succombent; mais bientôt le virus paraît devenir moins actif et les moutons résistent à son action, tandis que les lapins périssent encore, mais après une maladie de plus en plus prolongée. Après un temps plus long, la culture faite à 42° n'est plus dangereuse pour le lapin, elle tue encore le cochon-d'Inde et les souris, mais il arrive un moment où elle devient inoffensive même pour ces petits rongeurs si sensibles à l'action du virus charbonneux. Dans cette expérience comme dans celle du choléra des poules, nous voyons le virus du charbon prendre tous les degrés de virulence décroissante jusqu'à devenir inoffensif. La bactéridie cultivée à haute température et qui ne donne pas de germes dans ces conditions, en forme rapidement si on la cultive à 30° ou 35°. Les spores qui se produisent alors conservent la virulence du filament bactéridien qui les a produites, de sorte qu'il suffit de puiser chaque jour un peu de semence dans le flacon à 42° et de la porter dans du bouillon à la température de 35°, pour avoir une série de cultures de virulences graduées, cultures pourvues de germes qui fixeront chacune de ces virulences spéciales.

Les moutons et les bœufs qui reçoivent ces bactéridies atténuées éprouvent une fièvre passagère, et si plus tard on les inocule avec du virus virulent ils n'ont aucun mal. Il n'y a donc qu'à choisir dans cette échelle de virulence celle qui donne à l'animal que l'on veut

préserver du charbon une maladie légère mais suffisante pour lui conférer l'immunité. Dans la pratique, la vaccination des moutons et des bœufs se fait en deux fois. Le virus qui sert à la première inoculation est très atténué, il a pour effet de préparer les animaux à l'action du second vaccin plus énergique qui est inoculé douze jours après le premier. Toute la difficulté de la vaccination charbonneuse consiste dans le choix de ces deux virus et dans le maintien de leur virulence dans un rapport invariable.

Tout le monde a encore présente à l'esprit la démonstration éclatante de l'efficacité de ces inoculations préventives qui fut faite à Pouilly-le-Fort en 1881. Vingt-cinq moutons, pris au hasard dans un lot de cinquante animaux, reçurent les virus atténués du charbon, puis ils furent inoculés avec le virus virulent en même temps que les vingt-cinq moutons neufs restants qui servaient de témoins. Les vingt-cinq moutons vaccinés demeurèrent bien portants, les vingt-cinq moutons neufs périrent du charbon. Quelque démonstrative que fut cette expérience, elle ne prévint pas les attaques. Ce fut d'abord le principe même de la méthode qui fut contesté. On nia l'atténuation du virus charbonneux dans les conditions précisées par M. Pasteur. Il n'y a pas à s'arrêter aujourd'hui à cette objection, elle est abandonnée par ceux qui l'ont soulevée, et l'atténuation de la bactéridie cultivée à 42° est devenue une vérité classique. Ce qui fut ensuite mis en doute, c'est l'efficacité pratique des inoculations anticharbonneuses. La réponse à cette critique est écrite ici, sur ces tableaux qui indiquent le chiffre des animaux inoculés en France depuis 1881.

Animaux vaccinés contre le Charbon.

Années.	Moutons.	Bœufs.	Mortalité sur les moutons.
1882	243,199	22,916	1·08 pour cent
1883	193,119	20,501	0·77 „
1884	231,693	22,616	0·97 „
1885	280,107	21,073	0·90 „
1886	202,064	22,113	0·75 „
1887	293,572	42,538	
1888	269,599	34,464	

La perte moyenne en moutons, avant la vaccination charbonneuse, était de 10 pour cent, aujourd'hui elle est de moins de 1 pour cent.

Vous voyez combien ces inoculations préventives sont entrées dans la pratique agricole ; l'extension qu'elles ont prise est la meilleure preuve de leur efficacité. Le cultivateur, en effet, n'a pas souci des discussions scientifiques ; il n'engage pas l'existence de son bétail par esprit de théorie. Pour lui, la vaccination préventive contre le charbon est jugée par la balance des profits et pertes, et depuis huit ans il a pu se

faire une opinion en connaissance de cause. La mortalité des moutons s'est abaissée, dans les pays à charbon, de 10 pour cent à 1 pour cent ; dans ces contrées, les compagnies d'assurances inscrivent dans leurs contrats une clause qui oblige à inoculer préventivement le bétail assuré. En Autriche, en Italie, en Espagne la vaccination charbonneuse commence à être pratiquée dans de grandes proportions. Quels arguments pourrait-on ajouter qui établissent mieux les services qu'elle rend à l'agriculture ?

Mais ce n'est pas sur ces résultats pratiques que je veux insister ; après avoir parlé de l'atténuation des virus, je voudrais vous entretenir du retour à la virulence de ces virus atténués. Si nous rencontrions dans la nature une de ces bactéridies atténuées, que nous avons appris à préparer et qui sont si dépourvues de virulence qu'elles ne peuvent plus tuer même une souris, il nous serait évidemment impossible de reconnaître dans cet être microscopique inoffensif le descendant de la redoutable bactéridie charbonneuse. Il faut avoir assisté aux phases graduelles de son atténuation pour savoir que ce bacille anodin vient du bacille mortel. Il est cependant possible (si l'atténuation n'a pas été poussée trop loin) de faire remonter à cette bactéridie l'échelle de virulence qu'elle a descendu, et de la rendre de nouveau nocive pour les animaux. Nous avons dit qu'elle était incapable de tuer une souris adulte, inoculons-la à une souris très-jeune, âgée de un jour seulement. Cette jeune souris est plus sensible à l'action du virus qu'une souris adulte, elle périra après quelques jours. En se cultivant dans cette jeune souris la bactéridie aura repris un peu de son aptitude ancienne à vivre dans les milieux vivants, et le sang de la première souris inoculé à une seconde un peu plus âgée amenera la mort. Si bien, qu'en allant ainsi prudemment de souris plus jeunes à des souris plus vieilles, on arrivera à tuer les souris adultes, puis les cobayes, puis les lapins, puis les moutons, enfin les bœufs qui sont parmi les plus résistants des animaux qui prennent le charbon.

Voici donc que la virulence a été augmentée comme elle a été diminuée, et qu'elle se manifeste à nous comme l'aptitude des microbes à pulluler dans le corps des animaux vivants, aptitude qui peut s'acquérir et se perdre dans des conditions données. Nous concevons fort bien que cet accroissement de la virulence, que nous venons de réaliser expérimentalement, puisse se faire à notre insu dans la nature, et qu'un microbe, d'abord inoffensif pour une espèce animale, devienne nuisible pour elle. Il suffira, qu'introduit par une circonstance fortuite dans un être moins résistant, il s'y cultive. Cette première culture commence son adaptation à la vie parasitaire, il en sort prêt à pulluler dans un organisme qu'il n'aurait pu envahir auparavant, et après plusieurs passages successifs il sera devenu redoutable.

Il n'est pas téméraire de penser que, par ce procédé, se sont créées dans le cours des âges des virulences nouvelles, et que ces expériences sur les variations de la virulence jettent une vive lumière sur cette question si obscure : Comment naissent les maladies virulentes ? Elles nous expliquent aussi comment un même microbe peut causer des manifestations morbides diverses, comment, très actif, il donne une maladie générale rapidement mortelle, et comment, atténué, il ne fait plus qu'une lésion qui reste locale.

D'autres virus ont été atténués par cette méthode de l'action de l'air sur les cultures. Au Congrès tenu à Genève en 1882, M. Pasteur citait toute une série d'exemples nouveaux. Je ne vous parlerai que du virus atténué d'une maladie des porcs, très redoutée par les éleveurs, et connue en France sous le nom de *rouget*, parce que les animaux qui en sont atteints ont avant leur mort des taches rouges sur la peau.

Le microbe du rouget est un bacille qui se rencontre dans la rate et dans les ganglions lymphatiques des porcs qui succombent à la maladie. Dans les cultures il ne donne pas de germes, il sera donc particulièrement accessible à l'influence de l'air. Il perd en effet sa virulence quand on laisse les cultures assez longtemps exposées à l'air. Mais, il est une autre méthode d'atténuation du rouget découverte par MM. Pasteur et Thuillier, et qui a pour nous un grand intérêt parce qu'elle nous donne un exemple des changements que peut subir un virus dans son passage à travers des espèces animales différentes. Le rouget du porc injecté dans le sang d'un lapin le tue en quelques jours ; si avec un peu de la pulpe de la rate de ce premier lapin on inocule un second, celui-ci succombera plus rapidement encore. On peut ainsi faire passer le rouget par une série de lapins, et chose surprenante, dans les expériences de MM. Pasteur et Thuillier, à mesure que la virulence s'exalte pour le lapin elle diminue pour le porc. Si bien, qu'après un nombre suffisant de ces passages, le virus du rouget est atténué pour le porc. Il est devenu, pour cet animal, un vaccin véritable capable de le mettre à l'abri de la maladie mortelle.

Après cet exemple, ne peut-on pas se demander ce qui adviendrait de certaines maladies humaines si on les faisait ainsi passer à travers un grand nombre d'animaux d'espèces différentes ? Ces passages n'ont-ils pas eu lieu dans la nature à notre insu, et l'idée que la vaccine est la variole modifiée par le passage sur le cheval et la vache ne trouve-t-elle pas dans ces faits comme un nouvel appui ?

Après tous ces travaux sur le choléra des poules, le charbon et le rouget, c'est à prévenir la rage que M. Pasteur a consacré ses efforts dans ces dernières années.

Que savions-nous sur la rage, lorsqu'en 1880 l'étude de cette maladie fut commencée au laboratoire de M. Pasteur ? Nous savions que la

rage est contagieuse, que le virus rabique est contenu dans la salive des animaux enragés et que c'est par leurs morsures que ceux-ci transmettent la maladie. Nous savions encore que la durée de l'incubation varie de quelques jours à plusieurs mois. A ces notions se bornaient nos connaissances précises sur la rage. Cependant beaucoup d'expériences avaient été faites sur cette maladie, mais deux circonstances rendaient l'expérimentation difficile et les résultats incertains.

L'inoculation de la salive d'un animal rabique à un animal sain, ne donne pas toujours la rage; beaucoup de ces inoculations restent sans effet. Parmi les animaux qui prennent la maladie, quelques-uns deviennent enragés après un temps si long que cette attente prolongée met la patience de l'expérimentateur à une dure épreuve. La salive de l'animal enragé est un virus infidèle, parce qu'elle renferme une quantité de microbes variés, qui, introduits sous la peau en même temps que le virus rabique, empêchent le développement de celui-ci et le font disparaître dans une sorte de concurrence vitale. Le premier progrès à accomplir était de trouver une source de virus rabique non souillée de germes étrangers. Tous les symptômes de la rage relèvent du système nerveux et l'idée que le virus rabique doit exister dans les centres nerveux s'impose à l'esprit. Les tentatives faites pour montrer que la substance nerveuse d'un chien rabique est virulente n'avaient cependant pas réussi, parce que dans les manipulations que l'on faisait subir à cette matière nerveuse pour l'inoculer on y introduisait les germes étrangers qu'il fallait précisément éviter. En inoculant, avec pureté, de la moelle épinière, du cerveau, des nerfs d'un animal mort de rage, M. Pasteur montra que le véritable siége du virus rabique est dans la substance nerveuse. Une parcelle des centres nerveux d'un chien rabique, insérée sous la peau d'un chien en bonne santé, lui communique la rage, et cela plus sûrement que la salive la plus active. Cette démonstration permettait de faire un pas décisif dans l'étude de la maladie.

Puisque le virus rabique se trouve dans les centres nerveux et que tous les symptômes de la rage relèvent du système nerveux, n'était-il pas naturel de penser que la rage ne se manifeste que lorsque les centres nerveux sont envahis par le virus, et que la période d'incubation est le temps employé par le virus pour aller du lieu d'inoculation à l'axe cérébro-spinal et y faire sa culture? Si donc on porte ce virus tout d'abord dans le tissu nerveux, là où il doit se cultiver, l'incubation devra être abrégée et la rage devra apparaître à coup sûr, parceque le virus ne pourra plus s'égarer ou être détruit dans un long trajet.

L'expérience, Messieurs, a confirmé ces vues de l'esprit, et je vois encore ce premier chien inoculé à la surface du cerveau, par trépanation, et qui prit la rage après une incubation réduite à quatorze jours. Tout chien, en effet, qui reçoit sous la dure-mère un peu de la moelle

ou du cerveau d'un animal enragé prend sûrement la rage et dans un délai qui, en général, ne dépasse pas dix-huit jours. Nous voici donc désormais à l'abri des incertitudes de l'inoculation sous la peau et des ennuis des longues incubations. Aussi, après cette expérience, les progrès se multiplient dans l'étude de la rage, on prouve que le virus existe dans les nerfs et que par cette voie il va de la plaie au cerveau et à la moelle, qu'il peut aussi dans quelques cas être transporté par la voie sanguine. On comprend que les manifestations rabiques sont aussi variées qu'il y a de foyers fonctionnels divers dans les centres nerveux, que les symptômes de la rage au début dépendent de la région tout d'abord abordée par le virus, enfin l'on reconnaît qu'il existe des formes de rage jusqu'alors passées inaperçues et différentes des types classiques.

L'opération de la trépanation est par elle-même inoffensive, quand elle est faite avec des précautions antiseptiques. Elle réussit sur le lapin avec la même sûreté que sur le chien. Si on pratique l'inoculation de la rage, par trépanation, sur une série de lapins, en se servant du bulbe de l'animal qui vient de mourir pour inoculer le lapin suivant, on voit que la durée de l'incubation qui était de quatorze à dix-huit jours au début de l'expérience va en diminuant. Elle devient de plus en plus courte à mesure que le nombre des passages est plus grand ; après une centaine de ces inoculations successives, elle n'est plus que de sept jours, puis elle arrive à n'être plus que de six jours. Alors elle ne diminue plus ; le virus rabique, par cette culture répétée sur le lapin, semble avoir atteint sa virulence maximum pour cette espèce ; on dit qu'il est fixé. C'est de ce virus-fixe que M. Pasteur a tiré le virus-vaccin de la rage, par un procédé qui sur plus d'un point rappelle celui déjà employé pour atténuer le choléra des poules, le rouget et le charbon.

Dans un flacon à tubulure inférieure, contenant dans le fond des fragments de potasse caustique et fermé par des tampons de coton, suspendons une moelle rabique de lapin de passage. Cette moelle qui renferme en abondance le virus-fixe va se dessécher à l'abri des poussières, et au contact de l'air à 23°, car nous avons soin de la maintenir à cette température. Si chaque jour nous prélevons un fragment de cette moelle pour l'inoculer à la surface du cerveau d'un lapin, nous constaterons qu'à mesure que la moelle se dessèche dans l'air, elle perd sa virulence. Au bout de cinq jours de dessication elle ne tue déjà plus que quelques-uns des lapins qui l'ont reçue. Au bout de quatorze jours environ elle se montre tout-à-fait inoffensive, après avoir passé les jours précédents par des virulences graduellement décroissantes.

Maintenant que nous avons des virus rabiques atténués, injectons chaque jour sous la peau d'un chien un fragment de moelle atténuée broyée dans l'eau pure, en ayant soin de commencer par l'injection de la moelle inoffensive de quatorze jours et de continuer le second jour par

l'injection de la moelle de treize jours, puis le troisième jour par l'injection de la moelle de douze jours jusqu'à l'inoculation de la moelle de zéro jour, c'est-à-dire de la moelle non atténuée, de la moelle dont la virulence est mortelle. Ce chien ne succombe pas à la rage, bien plus, nous pouvons l'éprouver en lui inoculant dans le cerveau le virus rabique le plus actif, il ne devient pas malade. Et cependant nous savons que l'inoculation intra-crânienne est un procédé certain pour donner la rage. Les injections de moelles desséchées qu'il a subies lui ont donc conféré l'immunité. L'expérience peut être recommençée autant de fois qu'on le désire, le résultat sera toujours le même. Les chiens qui ont reçu sous la peau la série des moelles de quatorze à zéro jours ne prennent plus la rage, ni à la suite des morsures de chien enragé, ni autrement. L'état réfractaire a été ainsi obtenu en une quinzaine de jours.

D'ordinaire, la rage ne se déclare chez un chien mordu par un animal enragé, qu'après un délai qui le plus souvent dépasse un mois. Ne serait-il pas possible de profiter de cette longue incubation et de donner l'immunité contre la rage avant l'apparition de la maladie ? Des chiens furent mordus par des chiens enragés, ou furent inoculés sous la peau avec du virus rabique ; les uns furent conservés comme témoins, les autres furent soumis aux injections préventives des moelles desséchées de virulence croissante ; aucun de ces derniers ne prit la rage, tandis que les premiers moururent en grand nombre de la maladie caractérisée. Il était donc possible de prévenir la rage après morsure.

Malgré tous les résultats favorables obtenus sur les animaux, appliquer à l'homme mordu la méthode éprouvée sur le chien était assurément faire un pas audacieux. On sait par quelles sollicitations, par quels conseils autorisés M. Pasteur fut décidé à le franchir. Le 6 Juillet, 1885, l'enfant Meister mordu cruellement par un chien enragé, subissait la première inoculation antirabique. C'est là une date qui mérite d'être rappelée ; elle marque non seulement dans l'histoire du laboratoire de M. Pasteur, mais aussi dans celle de la science. Je ne dirai pas longuement comment à la suite de ce premier essai heureux, les mordus affluèrent de toute part au laboratoire, comment depuis cette époque, chaque mois 150 personnes environ viennent réclamer l'inoculation antirabique. Quelques-uns d'entre vous, Messieurs, ont assisté à ces inoculations, ils ont vu avec quel soin sont préparées les émulsions des moelles atténuées, pour éviter l'introduction de tout germe étranger. Les injections se font dans la région du flanc, à droite et à gauche alternativement ; elles sont répétées pendant quinze jours. Pour les morsures ordinaires on commence par l'injection de la moelle de quatorze jours et on s'arrête à celle de trois jours. Pour les morsures graves qui siégent à tête, on fait un plus grand nombre d'inoculations et on arrive plus rapidement aux moelles récentes, parce qu'on

n'a pas été longtemps à apprendre que, contre ces morsures, il fallait un traitement plus actif.

Depuis le début des inoculations antirabiques 6870* personnes ont été traitées dans le seul Institut de Paris.† Parmi elles, beaucoup avaient des morsures graves. La preuve que l'animal mordeur était enragé a été fournie soit par l'expérimentation soit par l'examen vétérinaire dans plus de 80 pour cent des cas. La mortalité par la rage, sur ces personnes traitées, est de 1 pour cent environ; elle est très faible si on la compare à celle de 14 pour cent qui suit d'ordinaire les morsures de chiens enragés. Qui aurait pu croire que ce nombre si petit d'insuccès serait l'occasion d'attaques violentes contre la pratique des inoculations antirabiques? Ces inoculations ont subi les reproches les plus divers: on les a accusées d'être inefficaces, et des contradicteurs avançaient que les bons résultats publiés étaient dûs à cette circonstance que le traitement dans presque tous les cas était appliqué à des personnes mordues par des chiens non-enragés. La statistique, disaient-ils, montre qu'en France il y a autant de morts par rage depuis l'invention du traitement antirabique qu'il y en avait avant. Cette assertion était celle d'hommes mal informés, qui prenaient pour des statistiques complètes des documents reconnus insuffisants par ceux mêmes qui les publiaient. Quant à la preuve de l'efficacité du traitement, elle se dégageait de l'examen des cas pour lesquels la rage de l'animal mordeur était prouvée expérimentalement, et surtout des résultats obtenus sur les personnes mordues à la figure. On sait en effet, qu'à la suite des morsures à la tête et à la face, la mortalité est de 80 pour cent au moins; chez les mordus de cette catégorie, traités à l'Institut Pasteur, elle n'atteint pas 4 pour cent.

D'autres adversaires soutenaient que le traitement était dangereux et augmentait les chances de mort. De sorte que l'on était en présence du fait singulier d'un traitement dangereux abaissant la mortalité dans des proportions inespérées. Ces premiers contradicteurs n'avaient pour soutenir leur cause que la force de leurs raisonnements, car ils n'avaient fait aucune expérience. Mais d'autres survinrent qui, par des expériences, voulurent prouver que le fondement même de la méthode était mal établi, et que les inoculations antirabiques ne donnaient pas l'immunité aux chiens. Vous savez ce qu'il est advenu de ces prétendues preuves expérimentales; elles ont été montrées inexactes, avec une autorité à laquelle on ne saurait ajouter, par les membres de la Commission Anglaise chargée de contrôler la méthode des inoculations antirabiques. Vous savez, Messieurs, de quels savants était composée cette Commission, et il

* Chiffre à la date du 21 Mai, 1889.

† Il existe 7 instituts antirabiques en Russie, 5 en Italie, 1 à Constantinople, 1 à Barcelonne, 1 à Bucarest, 1 à Rio de Janeiro, 1 à la Havana, 1 à Buenos Ayres, 1 à Mexico, 1 à Vienne.

suffit pour répondre définitivement à toutes les attaques de rappeler la conclusion de leur rapport, à savoir : que "M. Pasteur avait trouvé une méthode préventive de la rage comparable à celle de la vaccination contre la variole."*

Peut-on espérer que la mortalité chez les personnes mordues et traitées deviendra nulle ? Je ne le crois pas. Le plus grand nombre des personnes traitées, et qui ont succombé, ont pris la rage dans la quinzaine qui a suivi les inoculations. Cela tient à ce que chez elles, le virus a été apporté aux centres nerveux presqu'aussitôt après la morsure. L'expérience nous montre, en effet, que la rage éclate du douzième au dix-huitième jour après l'inoculation sous-méningée ; elle nous apprend aussi qu'il est très difficile de prévenir la rage chez les animaux ainsi inoculés, parce que la période d'incubation est si courte que les virus atténués injectés sous la peau, loin de l'axe nerveux, n'ont pas le temps d'agir. Dans les cas où l'incubation est très courte, le traitement peut donc être inefficace ; heureusement ces cas sont rares, même après les morsures à la figure. Quant aux insuccès exceptionnels, qui surviennent après que le traitement a été complet, et que son effet a eu le temps de se manifester, il est difficile de se rendre compte de leur cause, ils tiennent peut-être à une receptivité particulière.

Messieurs, ce qu'il y a de plus étonnant dans cette découverte de l'inoculation préventive de la rage c'est qu'elle a été faite sans que l'on connaisse le virus rabique. Non seulement nous ne savons pas cultiver ce virus hors de l'organisme, mais, si nous admettons qu'il est un microbe c'est par analogie, car personne n'a encore pu montrer ce microbe d'une façon certaine. Et cependant ce virus inconnu a été atténué, chaque jour on le prépare à des états variés de virulence. A défaut de cultures artificielles *in vitro*, M. Pasteur a fait la culture du virus rabique sur le lapin. Ces cultures sur l'animal vivant s'obtiennent avec une régularité si parfaite, une sécurité si grande, que chaque jour, pour le service des inoculations, elles sont préparées à l'heure dite, à l'état de véritables cultures pures. Il n'y a pas d'exemple plus saisissant de la puissance de la méthode expérimentale appliquée aux choses de la médecine que cette prophylaxie d'une maladie dont on ne connaît pas la véritable cause.

Cette série de découvertes sur la prévention des maladies contagieuses est l'œuvre d'un seul laboratoire, et a été accomplie en moins de dix années ; mais, quelque soit l'intérêt pratique de semblables travaux, il est de beaucoup dépassé par l'importance du mouvement scientifique dont ils ont été le point de départ. Ils ont permis d'aborder l'étude si compliquée de l'immunité, et je voudrais en

* Les membres de la Commission Anglaise étaient :—MM. James Paget, Lauder Brunton, George Fleming, Joseph Lister, Richard Quain, Henry E. Roscoe, Burdon Sanderson, Victor Horsley, secrétaire.

finissant vous dire quelques mots des plus récentes acquisitions faites sur ce sujet. C'est la conclusion naturelle de cette lecture sur les inoculations préventives. Avant de rechercher comment l'état réfractaire est produit, demandons-nous comment on meurt dans les maladies infectieuses ?

Certains microbes, celui du charbon, par exemple, pullulent tellement dans le corps des animaux, qu'il y a dans le sang, au moment de la mort, plus de cellules parasites que de globules sanguins. Les bactéridies forment parfois des obstructions capillaires et agissent ainsi mécaniquement. Mais, comme toutes les cellules vivantes, les microbes ont leurs exigences vitales et on conçoit, qu'avec leur nombre immense, ils doivent singulièrement modifier les milieux où ils se développent. La bactéridie du charbon, qui est très avide d'oxygène, prend ce gaz aux globules sanguins et amène ainsi l'asphyxie des tissus. Mais les microbes sont surtout dangereux par les produits toxiques qu'ils fabriquent. Une preuve frappante qu'il en est ainsi, nous est fournie par le bacille de la diphtérie. Ce bacille ne pénètre point dans l'intérieur des tissus, mais se cultive à la surface d'une muqueuse, pour ainsi dire en dehors du corps; cependant il amène la mort, parfois avec une effrayante rapidité. Dans ce cas il n'y a ni invasion du corps ni conflit de cellules; il y a empoisonnement au moyen d'un produit très actif élaboré au niveau de la fausse membrane. Il est difficile de trouver ces produits toxiques dans le corps d'un animal qui succombe à une maladie infectieuse. Le milieu si compliqué des tissus se prête mal à une semblable recherche; d'ailleurs, ces poisons y sont en très petite quantité, car pendant que l'animal reste vivant il les élimine en partie. C'est dans les cultures, *in vitro*, qu'il faut s'exercer à découvrir ces produits de l'activité des microbes pathogènes.

La première expérience faite sur le sujet est due à M. Pasteur. Pour savoir quelle était l'action sur les poules des produits élaborés par le microbe du choléra des poules dans les cultures, M. Pasteur injectait à ces animaux une grande quantité d'une culture absolument privée de microbes par filtration sur porcelaine. La poule qui avait reçu ce liquide, dépourvu de tout virus vivant, devenait somnolente, laissait pendre ses ailes, hérissait ses plumes et pendant plusieurs heures présentait tous les symptômes du choléra, puis elle recouvrait la santé. Cette expérience nous montre que les produits chimiques contenus dans la culture sont capables à eux seuls de provoquer les symptômes de la maladie, il est donc très probable que les mêmes produits sont préparés par le microbe dans le corps même des poules atteintes du choléra. Depuis, on a montré que beaucoup de microbes pathogènes faisaient de ces produits toxiques. Le microbe de la fièvre typhoïde, celui du choléra, celui du pus bleu, celui de la septicémie expérimentale aigüe, celui de la diphtérie, sont grands producteurs de poisons. Les cultures du bacille de la diphtérie notamment sont, au

bout d'un certain temps, si chargées du principe toxique, que privées de microbes, elles causent à des doses infiniment petites, la mort des animaux avec tous les signes que l'on observe après l'inoculation du microbe lui-même. Rien ne manque au tableau de la maladie, pas même les paralysies consécutives, si la dose injectée est trop faible pour amener une mort rapide. Dans les maladies infectieuses la mort survient donc par intoxication, le microbe est non seulement agent de la contagion, mais aussi préparateur de poisons.

Lorsqu'on introduit peu à peu, dans le corps des animaux, de ces substances chimiques préparées par un microbe pathogène, celui de la septicémie aigüe, par exemple, de façon à ne pas produire un empoisonnement brusque, mais une sorte d'accoutumance, ils deviennent réfractaires non seulement à l'action de doses toxiques qui les auraient tués tout d'abord, mais aussi à celle du microbe lui-même. L'immunité que nous ne savions donner jusqu'ici, qu'au prix de l'introduction d'un virus vivant dans le corps, peut donc être conférée par l'introduction d'un corps chimique dans les tissus. Ces substances vaccinales sont justement celles que nous avons vu causer la mort dans la maladie infectieuse; à forte dose elles tuent, à doses ménagées elles donnent l'immunité. Ces expériences de "vaccination" au moyen de matières solubles, sans microbes, ont déjà réussi pour diverses maladies infectieuses, et il est permis de croire qu'elles seront étendues bientôt à plus grand nombre encore.* Elles nous font comprendre la possibilité de la préservation d'une maladie par une autre; il suffit pour qu'il en soit ainsi, que les microbes de ces deux maladies élaborent des substances chimiques semblables.

Un animal, qui a reçu une dose suffisante de ces produits, est-il devenu réfractaire parce que ceux-ci restent présents dans les tissus et empêchent le développement du virus? On sait, en effet, que la croissance de certains microbes est arrêtée, dans les cultures, par l'accumulation des produits qu'ils y forment. Mais il faut se garder de conclure ce qui se passe dans les êtres vivants de ce qui se fait dans nos flacons. Retirons du corps d'un animal réfractaire au charbon, par exemple, un peu de son sang et ensemençons-le avec de la bactéridie charbonneuse. La culture sera abondante et rapide. Il n'y a donc pas dans le sang de ce mouton réfractaire de matière capable d'empêcher la vie de la bactéridie. Cette expérience est, il est vrai, tout-à-fait grossière, car il y a, au point de vue chimique, une différence énorme entre le sang contenu dans les vaisseaux d'un animal vivant et ce

* Je rappellerai ici les travaux de M. Salmon sur le choléra hog; de MM. Toussaint, Chauveau, Wooldridge, Chamberland et Roux sur le charbon; de M. Charrin sur la maladie pyocyanique; de MM. Chamberland et Roux sur la septicémie aigüe; de MM. Beumer, Brieger, Chantemesse et Widal sur la fièvre typhoïde; de M. Roux sur le charbon symptomatique, qui ont établi la vaccination par les substances chimiques élaborées par les microbes.

même sang retiré du corps et déposé dans un flacon. Si elle donnait un résultat, c'est que l'état réfractaire serait dû à un changement chimique véritablement énorme dans la composition des tissus. Pour la faire d'une manière plus délicate, injectons dans la chambre antérieure de l'œil du même mouton réfractaire au charbon, un peu de bactéridie virulente. La culture se fait très-bien dans l'humeur aqueuse, mais elle y reste localisée.* Il n'y a donc, dans cette humeur aqueuse, qui fait cependant partie du corps de l'animal, et qui participe aux modifications chimiques qui ont pu survenir en lui, il n'y a donc pas de substance capable de s'opposer à la vie du bacillus anthracis. Outre la question chimique il existe la question physiologique, ainsi que le prouve l'expérience suivante. Si on injecte du virus du charbon symptomatique dans la cuisse d'un lapin, animal naturellement réfractaire à cette maladie, aucune tumeur ne se développera, l'immunité paraît donc complète. Produisons maintenant par un choc, ou par l'injection d'une substance caustique, une lésion des tissus et faisons en ce point l'inoculation du virus, une tumeur charbonneuse apparaît bientôt, et quoique le lapin ne prenne pas d'ordinaire le charbon symptomatique, il peut arriver qu'il succombe. C'est que dans ce cas les tissus détruits ont formé comme un milieu inerte où le microbe a pu commencer sa culture sans obstacle. L'immunité des lapins contre le charbon symptomatique ne tient donc pas à ce que leur corps constitue un milieu impropre à la culture du virus, puisque celui-ci, grâce à un artifice d'inoculation, a pu l'envahir. Par des procédés semblables on peut aussi vaincre l'immunité acquise.

Que se passe-t-il donc quand on injecte du virus actif dans les tissus d'un animal réfractaire? Que deviennent les microbes? M. Metchnikoff nous a appris qu'ils sont bientôt détruits et que les agents de cette destruction sont surtout les globules blancs qui englobent les microbes et les digèrent. Dans le corps des animaux non réfractaires les cellules blanches n'englobent pas les microbes, ou si elles essayent de le faire ceux-ci se développent quand même.

Une explication satisfaisante de l'immunité doit tenir compte de tous ces faits et faire la part de l'action des produits chimiques et de la résistance des cellules. Nous pensons qu'actuellement l'interprétation la meilleure est celle qui considère l'immunité comme l'accoutumance des cellules aux poisons sécrétés par les microbes.

Lorsqu'un virus commence à se développer dans le corps d'un animal capable de prendre la maladie, il forme son poison et quand les cellules blanches viennent entreprendre la lutte, leur activité est entravée par cette production toxique, le microbe poursuit sa culture et la maladie progresse. Dans le corps d'un animal devenu réfractaire par injection préalable de substances solubles ou par inoculation antérieure de virus atténué, les cellules ont déjà été

* Expérience due à M. Metchnikoff.

accoutumées au poison microbien. Les doses faibles qu'elles trouvent au début de la culture du virus n'arrêtent pas leur action, elles entrent en lutte et digèrent le parasite. Mais si, comme dans l'expérience du charbon symptomatique sur le lapin, une circonstance empêche l'intervention cellulaire, la culture microbienne se fera, et dans ce foyer local il y aura bientôt assez de toxique préparé, pour que, malgré leur accoutumance préalable ou leur résistance naturelle, les cellules qui l'environnent soient réduites à l'impuissance. On comprend, en effet, qu'il ne puisse y avoir d'accoutumance pour les doses massives. C'est donc dans le temps qui suit immédiatement l'inoculation que se passe la lutte décisive. On conçoit alors l'importance du siége de l'inoculation et de la quantité de matière virulente introduite.

Lorsque nous connaîtrons bien les substances toxiques que forment les microbes pathogènes, nous pourrons peut-être leur trouver des contrepoisons qui paralyseront leur action au sein même des tissus. Mais je m'aperçois que, depuis un instant déjà, j'ai quitté le domaine des faits pour entrer dans celui de l'hypothèse et qu'il est temps que je m'arrête.

Cette façon de comprendre l'immunité concilie, je crois, les travaux multipliés dans ces dernières années. Il est probable que le temps la modifiera, mais ce qu'il ne changera pas c'est la reconnaissance de de tous pour celui qui par ses études sur les virus atténués et les vaccinations préventives, a permis d'aborder avec succès ce problême de l'immunité resté jusqu'ici impénétrable.

The Society adjourned over Ascension Day to Thursday, June 6th.

Presents, May 23, 1889.

Transactions.

Göttingen:—Königl. Gesellschaft der Wissenschaften. Nachrichten. 1888. 8vo. *Göttingen.* The Society.

Kew:—Royal Gardens. Bulletin of Miscellaneous Information. No. 29. 8vo. *London* 1889. The Director.

London:—Anthropological Institute. Journal. Vol. XVIII. No. 4. 8vo. *London* 1889. The Institute.

Institute of Chemistry of Great Britain and Ireland. Register. 1889. 8vo. *London.* The Institute.

Odontological Society of Great Britain. Transactions. Vol. XXI. No. 6. 8vo. *London* 1889. The Society.

Photographic Society of Great Britain. Journal and Transactions. Vol. XIII. No. 7. 8vo. *London* 1889. The Society.

Royal United Service Institution. Journal. Vol. XXXIII. No. 147. 8vo. *London* 1889. The Institution.

Transactions (*continued*).

Victoria Institute. Journal of the Transactions. Vol. XXII. No. 88. 8vo. *London* 1889. The Institute.

Zoological Society. Report of the Council. 1888. 8vo. *London* 1889. The Society.

St. Petersburg:—Académie Impériale des Sciences. Bulletin. Nouvelle Série. No. 1. 8vo. *St.-Pétersbourg* 1889. The Academy.

Stockholm:—Kongl. Vetenskaps-Akademie. Öfversigt. 1889. No. 2. 8vo. *Stockholm.* The Academy.

Vienna:—K.K. Geologische Reichsanstalt. Verhandlungen. 1889. Nos. 4–6. 8vo. *Wien.* The Institute.

Observations and Reports.

Berlin:—Commission für die Beobachtung des Venus-Durchgangs. Die Venus-Durchgänge 1874 und 1882. Bericht über die Deutschen Beobachtungen. Bd. II. 4to. *Berlin* 1889. The Commission.

Buenos Ayres:—Censo General de la Ciudad de Buenos Aires. 1887. Tomo I. 8vo. *Buenos Aires* 1889. The Census Commission.

Calcutta:—Meteorological Observations made at Seven Stations in India. October—December, 1888. Folio. [*Calcutta.*] The Meteorological Office, Calcutta.

Cambridge, Mass.:—Harvard College Observatory. Annals. Vol. XVIII. Nos. 7–8. Vol. XX. Part 1. 4to. *Cambridge* [1888], 1889; Henry Draper Memorial. Third Annual Report of the Photographic Study of Stellar Spectra. 4to. *Cambridge* 1889. The Observatory.

Liverpool:—Free Public Library, Museum, and Walker Art Gallery. Annual Report. 1889. 8vo. *Liverpool.* The Committee.

London:—Local Government Board. Supplement in continuation of the Report of the Medical Officer for 1887. Folio. *London* 1889. The Medical Officer.

Meteorological Office. Hourly Readings. 1886. Part 2. April to June. 4to. *London* 1889; Quarterly Weather Report. 1879. Part 4. October to December. 4to. *London* 1889; Report of the International Meteorological Committee, Zürich, 1888. 8vo. *London* 1889. The Office.

Stationery Office:—Report of the Scientific Results of the Exploring Voyage of H.M.S. "Challenger." Vol. XXX. Zoology. 2 vols. Text and Plates. 4to. *London* 1889. The Stationery Office.

Observations, &c. (*continued*).

Melbourne:—Observatory. Monthly Record. September to October, 1888. 8vo. *Melbourne*; Twenty-third Report of the Board of Visitors. Folio. *Melbourne* 1888.
The Observatory.

Rome:—Osservatorio del Collegio Romano. Pontificia Università Gregoriana. Vol. XXVII. Num. 11–12. 4to. *Roma* 1888.
The Observatory.

Washington:—Treasury Department, U.S. Government. Annual Report of the Comptroller of the Currency. 1888. 8vo. *Washington.* The Comptroller.

Wellington:—Colonial Museum and Geological Survey of New Zealand. Meteorological Report, 1885, including Returns for 1883 and 1884, and Averages for previous years. 8vo. *Wellington.* The Director.

Williamstown, Mass.:—Hopkins Observatory, Williams College. Catalogue of North Polar Stars, Right Ascension for 1885.0. 4to. *Williamstown* 1888; The Development of Astronomy in the United States. A Discourse to commemorate the Fiftieth Anniversary of the Hopkins Observatory. By T. H. Safford. 8vo. *Williamstown* 1888. The Observatory.

NOTICES TO FELLOWS OF THE ROYAL SOCIETY.

A Monograph of the Horny Sponges, by Dr. R. von Lendenfeld, is now ready for publication. The price to the public will be £3; to Fellows of the Royal Society, £2. As the edition is limited to 500 copies, Fellows should order their copies at once of the Assistant Secretary.

The following are now offered *to Fellows only* at reduced prices :—
"Catalogue of the Scientific Books in the Library of the Royal Society"—

Part 1.	Transactions, Journals, &c......	4*s*. net.
Part 2.	General Catalogue	7*s*. ,,
	The two Parts	10*s*. ,,

"International Polar Expedition. Fort Rae Observations." 10*s*. 6*d*. net.
"Catalogue of Scientific Papers," in sets :—

	Net prices. £	*s*.	*d*.	
Vols. I—VI in Half-Morocco at	3	10	0	the set.
*Vol. I, Half-Morocco, and II—VI, Cloth. .	2	14	6	,,
Vols. VII, VIII in Half-Morocco at . .	1	10	0	the two.
,, ,, ,, in Cloth	1	1	0	,,

The Library will be open during the months of August and September, from 11 A.M. to 4 P.M. (Saturdays 1 P.M.) No books can be given out during the month of August.

Philosophical Transactions.—The Fellows are now permitted to give standing orders for the delivery of the volumes of 'Philosophical Transactions' to their addresses as published. The order must be in writing, and should be sent to the Assistant Secretary.

On application to Messrs. Harrison and Sons, 46, St. Martin's Lane, Fellows and others can have the separate numbers of the 'Philosophical Transactions' bound in cloth case for 2*s*. 6*d*., or can obtain cloth cases for binding, price 1*s*. 6*d*.

List of Fellows.—Fellows who desire to have the Annual List posted to them every year as soon as issued should send their names to the Assistant Secretary.

A printed post-card of the papers to be read at each meeting will be sent weekly to any Fellow upon application to Messrs. Harrison and Sons, 46, St. Martin's Lane, W.C.

* Vol. I in cloth is sold out.

HARRISON AND SONS, 45 & 46, ST. MARTIN'S LANE, W.C.,

AND ALL BOOKSELLERS.

PROCEEDINGS OF

THE ROYAL SOCIETY.

VOL. XLVI. No. 282.

CONTENTS.

June 6, 1889.

Price Two Shillings and Sixpence.

June 6, 1889.

The Annual Meeting for the Election of Fellows was held this day.

Professor G. G. STOKES, LL.D., President, in the Chair.

The Statutes relating to the election of Fellows having been read, Dr. Hugo Müller and Mr. Stainton were, with the consent of the Society, nominated Scrutators to assist the Secretaries in examining the lists.

The votes of the Fellows present were then collected, and the following candidates were declared duly elected into the Society :—

Aitken, John.
Ballard, Edward, M.D.
Basset, Alfred Barnard, M.A.
Brown, Horace T., F.C.S.
Clark, Latimer, C.E.
Cunningham, Professor David Douglas, M.B.
Fletcher, Lazarus, M.A.
Hemsley, William Botting, A.L.S.
Hudson, Charles Thomas, LL.D.
Hughes, Professor Thomas McKenny, M.A.
Poulton, Edward B., M.A.
Sollas, Professor William Johnson, D.Sc.
Todd, Charles, M.A.
Tomlinson, Herbert, B.A.
Yeo, Professor Gerald F., M.D.

Thanks were given to the Scrutators.

June 6, 1889.

Professor G. G. STOKES, LL.D., President, in the Chair.

Professor John Milne (elected 1887) and Mr. Henry Trimen (elected 1888) were admitted into the Society.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

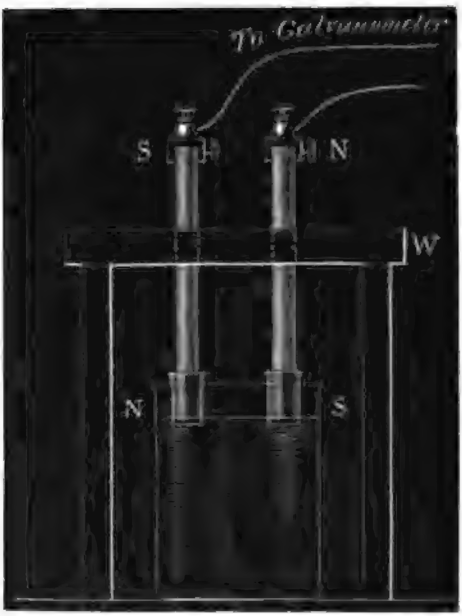
I. "Electro-chemical Effects on Magnetising Iron. Part III."* By THOMAS ANDREWS, F.R.SS.L. & E., M.Inst.C.E. Received May 3, 1889.

Experiments with Magnetic Polar Terminals.

In course of an earlier part of this research I made many preliminary experiments to investigate the possible electro-chemical effect between the polished end disks or alternate polar terminals of straight round steel magnets when immersed as elements in some electrolytes, only the north and south terminal planes of each steel magnet being simultaneously exposed to the action of the electrolyte.

Indications were afforded, under certain conditions, of a tendency on the part of the N. terminal of the magnet to become from some cause electro-positive to the S. terminal plane. The apparatus, fig. 6, was used in the first series of observations.

FIG. 6.



The magnets were placed parallel at some distance apart in an upright position, the lower end of each magnet, exposed in the solution,

* Parts I and II are printed at vol. 42, p. 459, and vol. 44, p. 152.

was covered with black india-rubber tubing, so that the flat polished terminal disks only were exposed to the action of the electrolyte. The bars used for the magnets were of polished steel, $4\frac{1}{4}$ inches long, $\frac{5}{16}$ inch diameter, cut adjacently, previous to magnetisation, from a longer bar. A pair of magnets were securely placed in the wooden frame W, and the N. and S. terminals of each bar immersed in the solution placed below. A number of experiments were made with the arrangement, fig. 6, and with a galvanometer in circuit, using a pair of new steel magnets each time. In these observations the current appeared to flow from the N. to the S. polar terminal; care was exercised to ensure that the magnets were as near as practicable equally magnetised.

The results of these preliminary observations are recorded in Table E.

Table E.

Time from commencement of experiment.	Electro-chemical effect between north and south polar terminals of magnetised steel bars. E.M.F. in volt. The electro-chemical position of the north polar terminal was positive, except where otherwise indicated.			
	Column 1.	Column 2.	Column 3.	Column 4.
	Cupric chloride.	Cupric bromide.	Nitric acid, sp. gr. 1·42, and potassium bichromate (conc. solution) in equal volumes.	Nitric acid, sp. gr. 1·42, one-third, and potassium bichromate (conc. solution), two-thirds.
seconds.				
0				
15	0·001			
30	0·002	0·007	0·009	
45	0·002			
minutes.				
1	0·002	0·010	0·005	0·009
2	0·006	0·011	0·004	0·009
3	0·006	0·011	0·006	0·004
4	0·010	0·016	0·005	0·009
5	0·014	0·029	0·006	0·011
6	0·016	0·045	0·008	0·011
7	0·023	0·051	0·009	0·011
8	0·038	0·050	0·009	0·011
9	0·054	0·047	0·010	0·010
10	0·063	0·032	0·011	0·009
11				
12	0·051	0·032	0·011	0·009
13				
14				
15	0·023	0·029	0·011	0·009
16				
17	0·009			
18				
19				
20	0·006	0·029	0·010	0·027
21				
22				
23				
24				
25	0·006	0·040	0·013	0·014
26				
27				
28				
29				
30	0·009	0·031	0·018	0·018
35		0·021	0·025	0·030
40		0·033	0·026	
45			0·041	
47½			0·132	
50			0·087	
55			0·054	
hour.				
1			0·054	0·018

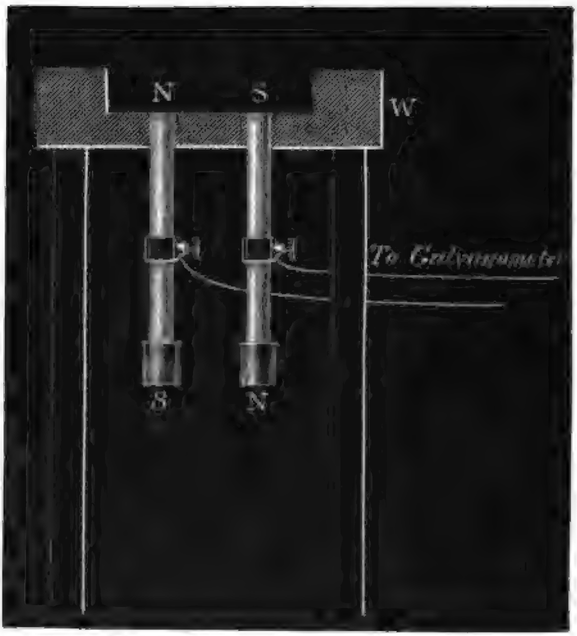
Table E—*continued.*

Time from commencement of experiment.	Electro-chemical effect between north and south polar terminals of magnetised steel bars. E.M.F. in volt. The electro-chemical position of the north polar terminal was positive, except where otherwise indicated.			
	Column 5.	Column 6.		
	Nitric acid and potassium bichromate solution.	Nitric acid, potassium bichromate solution, and ferric chloride solution.		
seconds.				
0		−0·009	0·030	−0·010
15				
30		−0·004	−0·030	0·018
45				
minutes.				
1	0·006	−0·004	0·038	0·011
2		0·004	0·009	0·009
3	−0·009	0·008	0·000	0·038
4	−0·009	0·004	0·009	0·023
5	0·000	0·004	0·023	
6	0·004	0·008	0·009	0·023
7	0·006	0·009		0·027
8	0·009	0·006	0·004	0·011
9	0·009	0·009	0·004	0·000
10	0·010	0·004	0·010	
11	0·018	0·004	0·010	0·009
12	0·014	0·006	0·006	0·006
13	0·011	0·011	0·006	0·004
14	0·027	0·011	0·004	0·004
15	0·054	0·014	0·006	0·004
16	0·158	0·014	0·006	0·004
17	0·271	0·018	0·009	0·004
18		0·013	0·011	0·009
19		0·011	0·010	
20		0·010	0·006	
21		0·004	0·006	0·002
22			0·004	0·004
23			0·009	0·004
24			0·009	0·006
25		0·004	0·004	0·004
26			0·004	0·009
27		0·004	0·006	0·004
28			0·010	0·006
29			0·009	
30			0·009	0·000
35			0·010	
40				
45			0·014	
47½				
50				
55				
hour.				
1				

The preceding indications of the apparent tendency of the N. pole to become electro-positive in an electrolyte appeared somewhat singular, and after communicating these preliminary results to Professor G. G. Stokes, and conferring with him thereon, it was decided to make further experiments, and in course of these I have endeavoured to utilise some valuable experimental suggestions which Professor Stokes kindly made.

Professor Stokes suggested that the results noticed in the first set of experiments with apparatus fig. 6 (see Table E) might probably be accounted for in the following manner, thus: supposing the bars to be equally magnetised permanently, then when the magnet bars were placed in the upright position the magnetism induced in the bars by the earth's magnetic force would in one magnet strengthen, and in the other oppose the permanent magnetism, so that the stronger pole would be the N. one at the bottom. To investigate this possible aspect of the matter, the apparatus, fig. 7, was constructed with which

Fig. 7.



to conduct the further investigations. The arrangement consisted of a wooden stand, W, the thick upper cross-bar of which was hollow and formed a tank sufficiently capacious to hold a suitable quantity of the electrolyte. The ends of two magnetised steel bars were securely inserted from below through the two holes in the bottom, so that the

upper terminal disks only of the magnets were exposed to the solution. These holes were very accurately and carefully drilled, and when the bars were forced in, the arrangement was quite fluid-tight, and the greatest care was exercised to ensure that the terminal planes only of the magnets were exposed to the action of the solution. The steel rods were connected with a galvanometer which was introduced into the circuit, and the electrolyte was then poured into the upper receptacle and the readings of the galvanometer noted. A new wooden stand was prepared for each experiment, and a new pair of steel magnets was also employed for each observation. In many experiments the electrolyte was first introduced to the lower ends of the bars, and observations first taken of the relative electro-chemical positions of the N. and S. terminals at the lower end of the magnets. The solution was afterwards removed, the lower bar ends cleaned, and the electrolyte subsequently placed in the upper receptacle. By this means an indication of the relative electro-chemical position of both the upper and lower polar disks of the same pair of steel magnets was afforded. This series of experiments are recorded in Table F, Sets I to IV, and in Table H, sets V and VI.

In other instances either the upper or lower polar terminals only of each pair of magnets were immersed in the electrolyte. The typical results of a considerable number of observations made in various ways in the above manner are recorded in Tables F, G, H, I, and J.

Explanation of Results on Tables F, G, H, I, and J.

The results on Table F, Sets I to IV, with cupric chloride as electrolyte, are the averages of numerous experiments on steel magnets which were tested in pairs with the electrolyte below, in apparatus fig. 7, the same pairs being afterwards tested at their opposite terminals with the electrolyte above.

The results on Table G, Experiments Nos. 62, 57, 56, 55, 46, 43, 42, and 47, with cupric chloride as electrolyte, are typical experiments selected from a large number of observations too numerous to record in detail. In these experiments the electrolyte was placed in the position described on the table, either above or below only, with each pair of magnets under observation.

The results on Table H, Sets V and VI, with cupric sulphate as electrolyte, are some typical observations selected from numerous experiments, in which the steel magnets were tested in pairs with the electrolyte below in apparatus fig. 7. The same pairs of magnets were subsequently tested at the opposite ends with the electrolyte above the magnets. The Experiments Nos. 71, 73, 75, and 79, on this table, with cupric bromide as electrolyte, were made with the solution below, experiments were also made with pairs of magnets placed in apparatus, fig. 7, and the results observed with the electrolyte above.

Table F.

Time from commencement of experiment.	Electro-chemical effect between north and south polar terminals of magnetised steel bars. E.M.F. in volt. The electro-chemical position of the north polar terminal was positive, except where otherwise indicated.			
	Cupric chloride solution.			
	Set I. Average of 10 experiments.		Set II. Average of 10 experiments.	
	Electrolyte below.	Electrolyte above.	Electrolyte below.	Electrolyte above.
seconds.				
0	0·010	−0·018	−0·011	−0·007
30	0·004	0·011	0·001	0·001
minutes.				
1	0·007	0·000	0·006	0·005
2	0·004	0·004	0·008	0·007
3	0·010	0·004	0·014	0·012
4	0·016	0·009	0·021	0·014
5	0·011	0·014	0·018	0·018
6	0·016	0·011	0·016	0·021
7	0·017	0·015	0·016	0·026
8	0·020	0·012	0·013	0·027
9	0·021	0·014	0·013	0·026
10	0·014	0·020	0·015	0·024
11	0·025	0·014	0·017	0·022
12	0·021	0·025	0·016	0·021
13	0·046	0·011	0·019	0·018
14	0·028	0·012	0·019	0·017
15	0·025	0·020	0·021	0·015
16				
17	0·032	0·011	0·021	0·015
18				
19				
20	0·024	0·018	0·023	0·015
21				
22				
23				
24				
25	0·025	0·014	0·022	0·015
26				
27				
28				
29				
30	0·025	0·013	0·023	0·017
35	0·026		0·031	0·009
40	0·036			
45	0·022			

Table F—*continued.*

Time from commencement of experiment.	Electro-chemical effect between north and south polar terminals of magnetised steel bars. E.M.F. in volt. The electro-chemical position of the north polar terminal was positive, except where otherwise indicated.			
	Cupric chloride solution.			
	Set III. Average of 10 experiments.		Set IV. Average of 8 experiments.	
	Electrolyte below.	Electrolyte above.	Electrolyte below.	Electrolyte above.
seconds.				
0	−0·002	0·001	0·010	−0·022
30	0·006	0·009	0·003	0·005
minutes.				
1	0·003	0·005	0·004	0·002
2	0·003	0·005	0·005	0·005
3	0·007	0·007	0·006	0·011
4	0·011	0·009	0·006	0·012
5	0·013	0·009	0·009	0·009
6	0·016	0·008	0·010	0·009
7	0·018	0·007	0·008	0·007
8	0·016	0·008	0·010	0·012
9	0·016	0·008	0·012	0·008
10	0·016	0·010	0·011	0·007
11	0·015	0·008	0·012	0·002
12	0·013	0·008	0·012	0·008
13	0·015	0·010	0·013	0·005
14	0·016	0·015	0·015	0·009
15	0·016	0·015	0·014	0·007
16	0·018	0·013		
17	0·019	0·018	0·010	0·006
18	0·019	0·014		
19	0·014	0·013		
20	0·013	0·015	0·013	0·009
21	0·017	0·014		
22	0·013	0·013	0·020	0·004
23	0·014	0·012		
24	0·013	0·012		
25	0·011	0·012	0·007	0·008
26				
27	0·017	0·014		
28				
29				
30	0·013	0·011	0·007	0·011
35				0·018
40				0·021
45				0·013

The pairs of magnets in the experiments of Set I varied in length from 8½ inches, 6 inches, and 4½ inches. In Sets II, III, and IV, the magnets of each pair were 6 inches long

Table G.

Time from commencement of experiment.	Electro-chemical effect between north and south polar terminals of magnetised steel bars. E.M.F. in volt. The electro-chemical position of the north polar terminal was positive, except where otherwise indicated.			
	Cupric chloride solution.			
	Experiment No. 62.	Experiment No. 57.	Experiment No. 56.	Experiment No. 55.
	Electrolyte below.	Electrolyte below.	Electrolyte below.	Electrolyte below.
seconds.				
0	0·006			−0·009
30	0·006			0·000
minutes.				
1	0·009	0·004	0·004	0·002
2	0·009	0·010	0·006	0·009
3	0·010	0·028	0·010	0·016
4	0·011	0·044	0·016	0·014
5	0·002	0·038	0·020	0·014
6		0·034	0·025	0·013
7	0·002	0·032	0·028	0·006
8	0·013	0·023	0·027	0·013
9	0·011	0·034	0·020	0·014
10	0·009	0·027	0·023	0·023
11	0·011	0·038	0·038	0·018
12	0·018	0·023	0·032	0·018
13	0·025	0·027	0·034	0·013
14	0·018	0·030	0·038	0·018
15	0·023	0·023	0·038	0·011
16	0·011			
17	0·014	0·034	0·034	0·009
18	0·013			
19	0·014			
20	0·030	0·041	0·034	0·016
21	0·018			
22	0·027			
23	0·028			
24	0·027			
25	0·018	0·048	0·041	0·004
27½	0·023			
30	0·027	0·044	0·034	0·002
35		0·054		
hours.				
1				
1½				
2				

Table G—*continued.*

Time from commencement of experiment.	Electro-chemical effect between north and south polar terminals of magnetised steel bars. E.M.F. in volt. The electro-chemical position of the north polar terminal was positive, except where otherwise indicated.			
	Cupric chloride solution.			
	Experiment No. 46.	Experiment No. 43.	Experiment No. 42.	Experiment No. 47.
	Electrolyte above.	Electrolyte above.	Electrolyte above.	Electrolyte above.
seconds.				
0	−0·027	−0·018	−0·009	−0·009
30	−0·004	0·000	0·000	
minutes.				
1	−0·003	0·002	0·003	
2	0·001	0·004	0·003	
3	0·002	0·009	0·006	
4	0·004	0·011	0·013	0·004
5	0·000	0·013	0·014	0·003
6		0·014	0·018	0·005
7		0·014	0·018	0·005
8		0·023	0·014	0·006
9		0·023	0·011	0·006
10		0·023	0·011	0·004
11		0·014	0·010	0·003
12	0·002	0·014	0·011	0·002
13	0·004	0·013	0·011	0·000
14	0·006	0·013	0·011	0·002
15	0·006	0·011	0·011	0·005
16				0·005
17	0·006	0·010	0·010	0·006
18				0·005
19				0·004
20	0·005	0·011	0·010	0·003
21				0·002
22				0·000
23				
24				
25	0·002	0·010	0·014	
27½				
30	0·002	0·009	0·011	
35				
hours.				
1	0·014			
1½	0·011			
2	0·014			

The magnets for the experiments in Table G were 6 inches long.

Table H.

Time from commencement of experiment.	Electro-chemical effect between north and south polar terminals of magnetised steel bars. E.M.F. in volt. The electro-chemical position of the north polar terminal was positive, except where otherwise indicated. Cupric sulphate solution. Set V.		Set VI.	
	Electrolyte below.	Electrolyte above.	Electrolyte below.	Electrolyte above.
seconds.				
0	0·009	−0·014	−0·011	0·004
30	0·006	−0·006	−0·004	0·006
minutes.				
1	0·006	0·000	−0·003	
2	0·007	0·004	0·000	
3	0·006	0·003	0·000	0·000
4	0·006	0·002	0·002	0·002
5	0·007	0·003	0·004	0·002
6	0·009	0·009	0·006	0·002
7	0·010	0·009	0·010	0·002
8	0·010	0·006	0·016	0·000
9	0·010	0·004	0·020	
10	0·010	0·003	0·023	
11	0·012	0·002	0·023	
12	0·018	0·000	0·023	0·004
13	0·016		0·025	0·009
14	0·020	0·000	0·028	0·010
15	0·041	0·000	0·030	0·011
16	0·054		0·030	0·011
17	0·110		0·027	0·009
18	0·122		0·018	0·009
19	0·054		0·016	0·014
20	0·028		0·016	0·015
21	0·011		0·014	0·016
22	0·009	0·000	0·027	0·014
23	0·000	0·009	0·041	0·018
24	0·004	0·010	0·018	0·038
25	0·004	0·010		0·030
26		0·011		0·014
27		0·014		0·004
28		0·018		0·009
29		0·025		0·009
30		0·038		0·010
31		0·054		
32		0·072		
33		0·110		
34		0·203		
35		0·226		
hours.				
2		0·006		

Table H—*continued.*

Time from commencement of experiment.	Electro-chemical effect between north and south polar terminals of magnetised steel bars. E.M.F. in volt. The electro-chemical position of the north polar terminal was positive, except where otherwise indicated.			
	Cupric bromide solution.			
	Experiment No. 71.	Experiment No. 73.	Experiment No. 75.	Experiment No. 79.
	Electrolyte below.	Electrolyte below.	Electrolyte below.	Electrolyte below.
seconds.				
0	0·009	0·006	−0·009	−0·004
30	0·004	0·010	−0·004	0·006
minutes.				
1		0·007	−0·001	0·006
2		0·006	0·003	0·007
3	0·000	0·011	0·004	0·006
4	0·002	0·014	0·002	0·006
5	0·005	0·014	0·004	0·009
6	0·007	0·020	0·003	0·009
7	0·009	0·024	0·003	0·007
8	0·009	0·025	0·003	0·007
9	0·006	0·027	0·004	0·006
10	0·007	0·025	0·005	0·005
11	0·007	0·028	0·003	0·004
12	0·009	0·028	0·009	0·003
13	0·009	0·032	0·010	0·002
14	0·010	0·036	0·011	0·001
15	0·011	0·038	0·010	0·001
16		0·040	0·011	0·002
17	0·014	0·040	0·009	0·003
18		0·044	0·009	0·003
19		0·044	0·006	0·004
20	0·016	0·043	0·009	0·004
21		0·043	0·006	0·005
22	0·020	0·043	0·004	0·006
23		0·041	0·003	0·005
24		0·041	0·006	0·003
25	0·019	0·041	0·005	0·004
26		0·041	0·005	0·003
27		0·040	0·002	0·003
28		0·040	0·004	0·003
29		0·041	0·004	0·005
30	0·015	0·041	0·006	0·005

The magnets for the experiments in Table H were 6 inches long.

Experiments were also made, using cupric bromide as an electrolyte, in which pairs of magnets 6 inches long were placed in apparatus, fig. 7, and the results observed when the electrolyte was thus above the magnets. The N. polar terminal was in the electro-positive position, with an E.M.F. somewhat similar in extent as when using cupric chloride.

Table I.

Time from commencement of experiment.	Electro-chemical effect between north and south polar terminals of magnetised steel bars. E.M.F. in volt. The electro-chemical position of the north polar terminal was positive, except where otherwise indicated.			
	Cupric sulphate solution.			
	Experiment No. 81.	Experiment No. 84.	Experiment No. 85.	Experiment No. 86.
	Electrolyte below.	Electrolyte below.	Electrolyte below.	Electrolyte below.
seconds.				
0	0·004	−0·018	0·010	−0·014
30	0·002	−0·011	0·009	−0·006
minutes.				
1	0·004	0·002	0·009	0·002
2	0·004	0·001	0·009	0·003
3	0·005	0·002	0·010	0·004
4	0·005	0·002	0·011	0·005
5	0·005	0·003	0·011	0·006
6	0·005	0·004	0·012	0·006
7	0·006	0·005	0·011	0·006
8	0·007	0·005	0·011	0·007
9	0·009	0·005	0·011	0·009
10	0·009	0·006	0·010	0·010
11	0·007	0·006	0·009	0·010
12	0·007	0·006	0·010	0·011
13	0·006	0·006	0·010	0·011
14	0·004	0·007	0·011	0·012
15	0·004	0·009	0·011	0·012
16	0·005	0·009	0·011	0·013
17	0·005	0·009	0·013	0·013
18	0·006	0·009	0·014	0·014
19	0·007	0·009	0·014	0·013
20	0·006	0·008	0·014	0·013
21	0·007	0·007	0·015	0·014
22	0·006	0·006	0·016	0·015
23	0·006	0·005	0·017	0·016
24	0·005	0·010	0·017	0·017
25	0·005	0·009	0·019	0·016
26	0·004	0·009	0·022	0·017
27	0·007	0·009	0·024	0·018
28	0·009	0·007	0·025	0·020
29	0·006	0·007	0·026	0·020
30	0·004	0·007	0·027	0·018

Table I—*continued.*

Time from commencement of experiment.	Electro-chemical effect between north and south polar terminals of magnetised steel bars. E.M.F. in volt. The electro-chemical position of the north polar terminal was positive, except where otherwise indicated.			
	Cupric sulphate solution.			
	Experiment No. 87.	Experiment No. 88.	Experiment No. 90.	Experiment No. 91.
	Electrolyte below.	Electrolyte below.	Electrolyte below.	Electrolyte below.
seconds.				
0	−0·018	−0·011	−0·014	0·018
30	−0·011	−0·004	−0·010	0·011
minutes.				
1	0·001	0·001	0·002	0·010
2	0·003	0·002	0·003	0·011
3	0·003	0·005	0·004	0·011
4	0·006	0·007	0·006	0·011
5	0·006	0·006	0·007	0·011
6	0·007	0·007	0·009	0·012
7	0·009	0·007	0·009	0·011
8	0·009	0·007	0·010	0·011
9	0·010	0·006	0·010	0·012
10	0·011	0·006	0·011	0·013
11	0·011	0·007	0·011	0·013
12	0·013	0·009	0·012	0·014
13	0·014	0·009	0·013	0·014
14	0·016	0·009	0·014	0·015
15	0·018	0·010	0·015	0·017
16	0·019	0·011	0·017	0·017
17	0·020	0·011	0·017	0·020
18	0·023	0·011	0·017	0·025
19	0·025	0·011	0·016	0·027
20	0·027	0·011	0·018	0·030
21	0·027	0·011	0·019	0·033
22	0·027	0·011	0·022	0·034
23	0·028	0·012	0·019	0·036
24	0·030	0·012	0·019	0·038
25	0·031	0·011	0·019	0·040
26	0·031	0·011	0·020	0·041
27	0·034	0·011	0·020	0·042
28	0·035	0·010	0·020	0·044
29	0·036	0·011	0·019	0·044
30	0·037	0·010	0·018	0·044

The magnets for the experiments in the above Table I were in some experiments 8½, and in others 4½ inches long.

Table J.

Time from commencement of experiment.	Electro-chemical effect between north and south polar terminals of magnetised steel bars. E.M.F. in volt. The electro-chemical position of the north polar terminal was positive, except where otherwise indicated.			
	A concentrated solution of potassium bichromate containing nitric acid.			
	Experiment No. 92.	Experiment No. 94.	Experiment No. 95.	Experiment No. 96.
	Electrolyte below.	Electrolyte below.	Electrolyte below.	Electrolyte below.
seconds.				
0	0·023	0·000	−0·009	−0·002
30	0·011	0·004	−0·004	0·004
minutes.				
1	0·006	0·009	−0·002	0·003
2	0·009	0·004	0·000	
3	0·009	0·004	0·006	0·003
4	0·011	0·010	0·011	0·006
5	0·011	0·009	0·009	0·009
6	0·009	0·009	0·010	0·011
7	0·006	0·006	0·011	0·009
8	0·009	0·004	0·018	0·002
9	0·004	0·010	0·014	
10	0·004	0·006	0·011	0·004
11		0·009	0·014	0·009
12		0·010	0·011	0·004
13		0·011	0·014	0·011
14		0·009	0·013	0·004
15	0·009	0·010	0·014	0·006
16		0·018	0·016	0·009
17	0·006	0·011	0·018	0·006
18		0·014	0·023	0·004
19		0·014	0·018	0·006
20	0·004	0·011	0·023	0·006
21		0·014	0·027	0·009
22		0·011	0·023	0·000
23		0·018	0·018	0·018
24		0·023	0·027	0·009
25		0·011	0·030	0·004
26		0·009	0·023	0·011
27		0·011	0·030	0·016
28		0·009	0·044	0·020
29		0·009	0·038	0·014
30		0·004	0·034	0·011
35				0·009

Table J—*continued.*

Time from commencement of experiment.	Electro-chemical effect between north and south polar terminals of magnetised steel bars. E.M.F. in volt. The electro-chemical position of the north polar terminal was positive, except where otherwise indicated.		
	Nitric acid, potassium bichromate solution, and ferric chloride solution.	Sulphate of iron (conc. solution) and ⅛th nitric acid.	
	Experiment No. 100.	Set VII.	
	Electrolyte below.	Electrolyte below.	Electrolyte above.
seconds.			
0	−0·009	0·072	0·011
30	0·014	−0·004	0·009
minutes.			
1		0·006	0·009
2		0·007	0·006
3	0·023	0·010	0·004
4	0·014	0·013	0·002
5	0·009	0·027	0·002
6	0·009	0·038	0·002
7	0·009	0·054	0·002
8	0·006	0·082	0·001
9	0·010	0·089	
10		0·079	
11	0·010	0·054	
12	0·009	0·038	
13	0·009	0·014	
14	0·006	0·004	
15	0·004		
16	0·009		
17	0·009		0·009
18	0·009		
19	0·009		
20	0·009		0·013
21	0·006		
22	0·014		
23	0·018		
24	0·011		
25	0·006		0·027
26	0·006		
27	0·009		
28	0·006		
29	0·004		
30	0·004		0·014
35			

The magnets for the observations in above Table J were 6 inches long.

Table I, with cupric sulphate as electrolyte. In these experiments Nos. 81, 84, 85, 86, 87, 88, 90 and 91, each pair of magnets was only tested with electrolyte below.

On Table J, in Experiments Nos. 92, 94, 95, and 96, with a solution of potassium bichromate and nitric acid as electrolyte, each pair of magnets was only tested with the electrolyte below. The Experiment No. 100, with a solution containing nitric acid, potassium bichromate, and ferric chloride as electrolyte, was made in apparatus fig. 7, with the electrolyte below. In the experiments, Set VII, with sulphate of iron and nitric acid, the magnets were tested first in apparatus fig. 7, with the electrolyte below, and subsequently the opposite ends were tested with the electrolyte above.

The diameter of the magnets in these experiments was about 0·300 inch.

In course of the investigations it was frequently noticed that an instantaneous fling of the galvanometer occurred on first immersing the bars in the electrolyte, the fling showing the S. polar terminal to be positive at the moment of commencing the experiment; this instantaneous fling of the galvanometer very rapidly subsided, and the subsequent readings showed a continuously steady positive position for the N. pole. (See results on the Tables.) It will also be generally noticed that the E.M.F. steadily increased from the commencement of an experiment, when the electrolyte was placed either above or below the magnets, the polar influence appearing gradually and increasingly to affect the action taking place. An examination of the results of the total observations, on the steel magnets, in Table F, in which the electrolyte (cupric chloride solution) was placed below the magnets, gave an average E.M.F. of 0·014 volt; whereas the observations made with the electrolyte above the magnets yielded an average E.M.F. of 0·011 volt. The further experiments in Tables G and H are also confirmatory of this difference in the extent of E.M.F.

These general results seem, therefore, to indicate that the positivity of the upper N. polar terminal was reduced relatively to some extent when the electrolyte was placed above the magnets as in fig. 7, and this circumstance may possibly be owing to the action of the upper N. pole having been comparatively weakened by the influence of the earth's magnetism, as suggested by Professor Stokes. This difference, ascertained from the results of such a considerable number of observations, in the extent of the E.M.F., may perhaps thus be accounted for. It will be seen that the united results obtained with both the arrangements of fig. 7 lead to the tentative conclusion that there was a general tendency, under the conditions of experimentation, on the part of the N. magnetic terminal of the steel magnets, to assume electro-chemically the positive position compared with the S. terminal, when these constituted elements in certain electrolytes. The result

was seemingly due to the magnetic poles diversely influencing the action of the solution on the metal; and in the case of the copper salts, the ratio of the electro-deposition of the copper was apparently affected by these magnetic influences. It may be remarked that this research has been replete with difficulties of various kinds, and about 386 magnets have been experimented upon in Part III with the various solutions to endeavour to ensure accuracy in the observations recorded.

II. "Report on the Effects of Contact Metamorphism exhibited by the Silurian Rocks near the Town of New Galloway, in the Southern Uplands of Scotland." By S. ALLPORT, F.G.S., and T. G. BONNEY, D.Sc., F.R.S.* Received May 7, 1889.

In the 'Transactions of the Royal Society of Edinburgh' (vol. 7, p. 79) is a paper by Sir John Hall, in which he says that he read to that Society, in the year 1790, an account of his observations on the granitic mass of Loch Ken. In this he stated that in all the "extent where the junction of the granite with the schistus was visible, veins of the former, from 50 yards to the tenth of an inch in width, were to be seen running into the latter in all directions, so as to put it beyond all doubt that the granite of those veins, and consequently of the great body itself, which I observed forming with the veins one continuous and uninterrupted mass, must have flowed in a liquid state into its present position."

Of the accuracy of these observations no competent observer can entertain a doubt, and microscopic examination of the rocks invaded reveals the great structural and mineralogical changes which have been produced by the intrusive granite. The portion of the altered Silurian strata examined by Mr. Allport forms a narrow band between the margin of Loch Ken and the granite *massif* of Cairn Muir and Cairn Fleet on the other. Starting from Lochside Point (about 2 miles south of New Galloway), and following a line drawn along the west side of the loch in a northerly direction, the altered

* The origin and authorship of this communication should perhaps be explained. In 1880 a grant was made by the Royal Society to Mr. S. Allport to enable him to study the phenomena of contact metamorphism in southern Scotland. Shortly before going there, he received an appointment at Mason's College, Birmingham, which for a while took up his whole time; and then a severe illness obliged him to lay aside for many months all scientific work. Last autumn, being still far from strong, he consulted me as to the best way of indicating that the above-named grant had not been without fruit, and, after some consideration, it was arranged that I should work over his specimens, and embody his notes with my own impressions in a report to the Royal Society.—T. G. B.

strata are seen always to strike towards the granite, and, in many cases, to be cut off sharply by it. The line of junction is far from being even or straight, for short dykes or masses of granite have been intruded between the beds.

The greater part of Mr. Allport's collection was obtained by following the line of junction, and represents the altered condition of the various beds through which the granite has broken. Hence it is reasonable to suppose that the differences in mineral character which they now exhibit are due to original differences in their constituents. Another group of specimens represents the rocks at a greater distance from any visible mass of granite. Of the former, Mr. Allport writes that the specimens indicate the nature of the rock, from its actual junction to a distance of about 12 yards from the granite, except in two instances, where the nearest mass of the latter was visible about 40 yards away. Here, however, the actual distance very probably is less, as the granitic mass slopes down beneath an intervening layer of turf.

Mr. Allport's collection contains some specimens from the granite *massif*, but it has not been thought necessary to have them sliced for microscopic examination. They show that, as might be expected, the rock varies considerably in texture and somewhat in mineral composition. Some are granophyres more or less porphyritic, others true granites. None are rich in quartz; one, in which biotite is abundant and a fragment of altered rock is included, is poor in this mineral. One contains small wine-red garnets. The dominant tint is some shade of grey.

Among clastic rocks, confining ourselves to the practically non-calcareous varieties, and putting aside the coarser breccias and conglomerates (none of which were seen by Mr. Allport in the district), we find every gradation from the finest clays either to pure quartz sandstones or to those miscellaneous grits which are often called greywackés. This old-fashioned and rather vague term may be conveniently used for a group of rocks especially common among the older Palæozoic strata of Britain.* These greywackés consist largely of quartz grains (often from 0·01" to 0·02" in diameter), with a fair proportion of fragments of about the same size or occasionally rather larger, among which the following may often be recognised:—(*a*) Composite quartz grains, including fragments of vein quartz and quartzite; (*b*) fragments of felspar of various species; (*c*) fragments of granitoid rock; (*d*) fragments of argillite and slate; (*e*) fragments of phyllite and fine-grained schists (generally mica-schist); (*f*) fragments of volcanic rock, glassy, cryptocrystalline, or scoriaceous. If one may generalise from a rather limited number of observations, the more acid lavas predominate over the basic, and among these

* Bonney, Address to Section C of the British Association, 1886 (Birmingham).

andesites or dacites seem more common. Mr. Allport's collection, as will be seen, represents the effects of contact metamorphism on rocks generally not rich in lime, and varying from silty clays to greywackés.

The minerals which have been produced by contact metamorphism, in the rocks now studied, do not require a lengthy description. In addition to what has been written in England, we are indebted to Professor Rosenbusch, Professor Barrois, and other foreign observers for some admirable studies.* Of these results, a very clear and full summary, up to a recent date, has been given by Mr. Teall,† so that it is needless to enter into details which may now be regarded as matters of common knowledge. The minerals in these specimens from Scotland are:—

(1) *Quartz*, which is very abundant, occurring both in small granules and in fair-sized grains, generally rather conspicuously free from inclusions.

(2) *Mica*, (*a*) brown, sometimes becoming greenish coloured, apparently from subsequent alteration,‡ (*b*) white.

(3) *Hornblende.* Occurs in grains of rather irregular outline, and not seldom interrupted by enclosures of quartz; sometimes rather acicular in habit and grouped. The length varies from about 0·01″ to 0·025″. Colour in thin sections a rather pale olive-green; pleochroism not very strongly marked; a, pale yellowish-green, b, olive-green or a rather deeper green. Some might certainly be called actinolite; the rest is nearer to one of the secondary hornblendes seen in epidiorites. Hornblende does not appear to be very common as a product of contact metamorphism in sedimentary rocks.

(4) *Augite.* Occurs in two or three slides with hornblende; in one case rather abundantly. In roundish or elongated grains of granular texture, almost colourless, or with the faintest possible tinge of greyish-green. Cleavage not very distinct, but sometimes grains show pretty clearly a cleavage parallel with ∞P, and then extinguish at angles often greater than 30°. Tints with crossed Nicols often brilliant. This mineral in general appearance resembles sahlite or malacolite, which are frequent contact products in limestone, so we may venture to regard it as a variety of augite.

* Lössen, 'Zeitsch. Deutsch. Geol. Gesell.,' vol. 19, p. 509; 21, p. 281; 24, p 701; Kayser, *ibid.*, vol. 22, p. 103; Zirkel, *ibid.*, p. 175; Fuchs, 'N. Jahrb. Mineral.' (1870), p. 742; Barrois, 'Ann. Soc. Géol. du Nord,' vol. 11, p. 103; Lévy, 'Bull. Soc. Géol. France,' sér. 3, vol. 9, p. 181; Hawes, 'Amer. Journ. Sci.,' vol. 21, 1881, p. 21.

† 'British Petrography,' Chapter xii.

‡ See below for some remarks on this mineral.

(5) *Garnet* (rare).

(6) *Epidote* (?) not common.

(7) *Black opaque minerals*:—Iron oxide and perhaps graphite.

Tourmaline is extremely raré, three grains occurring in one slide only, from close to a junction, and, what is more remarkable, no andalusite is distinctly recognisable. One or two other minerals, which may be regarded as "accidental," will be noticed in connexion with the specimens.

As the effect of contact metamorphism has been described in some detail by one or other of the present authors from the Lake District, Cornwall and Brittany, and by Dr. C. Barrois from the last-named country,* it will suffice to recapitulate very briefly the phenomena, (*a*) in the case of a shale, (*b*) in that of a sandstone.

(*a*.) Very commonly chiastolite is formed in comparatively large crystals, and this even before the matrix of the rock is very obviously affected. As the junction with the intrusive mass is approached, the chiastolite crystals frequently disappear, and rounded spots of andalusite, often very full of mineral enclosures, begin to occur. These after a time become clearer and more crystalliform in outline.† Simultaneously, the colouring matter of the rock collects in specks, rods, and ill-formed crystals, the size of the last increasing on getting near the junction. Mica, usually of a peculiarly rich brown colour, forms in distinct flakes, which are sometimes interrupted by granules of quartz, &c., sometimes locally darkened by black spots, with an indefinite outline, like an ink blot on porous paper. The basal cleavage is distinct, but the external angles are usually not well defined. These flakes sometimes exhibit a certain parallelism, and thus impart a foliation to the rock, but this commonly is not well marked. Quartz, generally in clear granules, is also developed; sometimes it is wholly of secondary origin, but in other cases, probably, original fragments are enlarged, till the rock has become a crystalline aggregate, chiefly consisting of quartz and mica. As a rule, white mica, at any rate in crystals of fair size, only makes its appearance very near to a junction.‡

This brown mica is evidently not a normal biotite. There are in its microscopic aspect certain peculiarities by which it may generally be distinguished; for instance, the dichroism and absorption are less strongly marked, the colour-change being from pale-yellow to reddish-

* Allport, 'Quart. Journ. Geol. Soc.,' vol. 32, p. 407; Bonney, 'Quart. Journ. Geol. Soc.,' vol. 44, p. 11; Barrois, 'Ann. Soc. Géol. du Nord,' vol. 11, p. 103; see also Ward, "Geol. of Lake District" ('Survey Memoirs'), p. 9.

† On this line of change, as it does not occur in the specimens before us, it is needless to dwell.

‡ It is interesting to observe in connexion with this that MM. Fouqué and Lévy failed to produce artificially, by fusion, white mica, but met with some success in the attempt to obtain biotite ('Synthèse des Minéraux et des Roches,' p. 126).

brown, indeed often it becomes deep brownish-black and opaque. So far as we know this mica has not been isolated, and made the subject of quantitative analysis, but an inference may be legitimately drawn from the published analyses of clays and of rocks affected by contact metamorphism, in which the presence of this mineral has been determined by microscopic examination.

The researches of Carius, Fuchs, and Unger* may be held to have demonstrated that, as a rule, no change of any importance occurs in the chemical composition of a mass of rock affected by contact metamorphism. If, then, we examine a series of analyses of ordinary argillaceous sediments, and of the rocks resulting from contact metamorphism of the same,† we are struck with their comparative poverty in magnesia and richness in iron oxides. For instance, in a group of nine analyses, we find the following ranges of constituents:—

MgO, from 0·06 to 2·596; the average being 1·185.

FeO, from 0·495 to 6·010 }
Fe_2O_3, from 3·380 to 8·165 } the two together commonly forming from 9 to 10 per cent. of the whole.

K_2O, from a trace to 3·765, but generally more than 1·2 per cent.

Na_2O, usually in much smaller quantity, but once rising to 2·170 per cent.

Every analysis, except one, gives more than 2 per cent. of alkalies, but this records only traces both of soda and of potash. The author estimates that in one of these rocks—the richest in magnesia—there is 32·4 per cent. of mica, but even in this example the percentage of Fe to Mg is 11 : 9. Most of the analyses indicate less than half this quantity of magnesia, so that the amount of the iron would be three or four times that of the other constituent; indeed, we may say that in eight of these rocks the iron must largely exceed the magnesia, and in one of them almost wholly replace it.

The inferences thus suggested are confirmed by other examples from the Pyrenees, from the Lake District, and elsewhere. It is then evident that very many—probably most—argillaceous rocks do not contain a sufficient amount of MgO to form an ordinary biotite in any quantity, so that we must suppose either that the constituent is subsequently introduced—which is highly improbable—or that the mica is an iron mica, not a ferro-magnesian mica. Such micas are known to exist. We do not indeed remember to have seen a separate analysis of the mica from a case of contact metamorphism, but in the table of analyses of black mica given by Mr. Teall,‡ we find one with MgO as low as 1·50 (FeO, 18·06; Fe_2O_3, 7·19), and similar micas are

* Fuchs, 'Neues Jahrb. f. Mineral.,' 1870, p. 742; Unger, *ibid.*, 1876, p. 785.
† Such as are given by Unger, 'Neues Jahrb. f. Mineral.,' 1876, p. 785.
‡ 'British Petrography.' p. 302.

analysed by Mr. F. W. Clarke,* from American localities. In fact, these contact-micas are either lepidomelanes or haughtonites, varietal names which are applied to iron micas, according as there is a predominance of Fe_2O_3 or FeO.†

The same analyses indicate that if a normal clay were converted into a quartz-mica rock, supposing all the alkalies present to be taken up in forming mica, a considerable proportion of alumina, and the larger part of the silica (something like 50 per cent.) would be left unused. If the alumina amounted to 11 per cent. of the whole, about 6·6 of the silica would be required for andalusite; this would leave not less than 43 per cent. of the silica free to form quartz. Thus from a normal clay, as the result of contact metamorphism, a rock is produced containing not less than 50 per cent. of free quartz.

(*b*.) After what has been said above, the history of a sandstone may be dismissed in few words. Almost all sandstones contain a certain amount of mud and silt. From these, both mica and subsilicates of alumina would form as before; but in this case the free silica would very commonly be deposited on the pre-existing granules or grains of quartz, probably in crystalline continuity with them, so that in many cases the fragmental aspect of the rock would be obliterated and its structure would resemble that of the quartz layers in the true crystalline schists.

In the above cases, if the original rock contained a lower proportion of alkalies and a fair one of lime, as well as some magnesia, we might expect minerals of the pyroxene group to be formed. If, however, felspar was present in fragments, and its constituents entered into new combinations, a white mica might be produced. A fragment of normal orthoclase, in forming muscovite, would liberate at least four-tenths of its silica,‡ and if it had lost some of its alkaline constituents by decomposition, it would be converted into a roughly equal mixture of quartz and muscovite.

Somewhat similar changes would occur in the case of fragments of an acid glass, which might be expected to produce, as the result of contact metamorphism, a microcrystalline mixture of quartz and white mica, while the alteration of a basaltic rock§ would give rise to pyroxenic minerals, epidote, probably some biotite or chlorite, with a smaller proportion of quartz.

On examining some of the slides in Mr. Allport's collection from

* 'Amer. Journ. Sci.,' vol. 34, 1887, p. 131.

† Mr. Allport ('Quart. Journ. Geol. Soc.,' vol. 32, p. 417) found that lithia was present in Cornish specimens of the brown contact-mica, but probably this is not an essential constituent.—T. G. B.

‡ Bonney, 'Quart. Journ. Geol. Soc.,' vol. 44, p. 37.

§ Allport, 'Quart. Journ. Geol. Soc.,' vol. 32, p. 407, &c.

Scotland, we are at once struck, notwithstanding the present crystalline condition of the rocks, with their resemblance to those cut from greywackés, which contain distinct fragments of quartz, felspar, and an argillaceous rock. We do not, indeed, observe an indubitable fragmental structure, but what we might term the ghost of one. For instance, the eye is at once arrested by subangular grains of quartz, which are much larger than those helping to form the mosaic of quartz and mica which constitutes the major part of the rock. Their edges are sometimes sharply limited by flakes of brown mica and are rectilinear, at others they join irregularly to the quartzes of the matrix, as if they had grown together. Sometimes these larger grains show a compound structure, but more often they are simple, part of a single crystal, just as may be seen in the fragments common in a greywacké. Usually they are fairly free from enclosures, though occasionally microliths and small fluid cavities with bubbles may be observed. Other patches, yet more shadowy in outline, consist of chalcedonic or microcrystalline quartz and flakelets of mica, generally white. Still, it is generally not difficult to distinguish them from the matrix, owing to their difference in composition and structure. Careful examination of these detects occasionally less altered portions in which the lamellar twinning of a plagioclase felspar is still distinctly recognisable. These, then, represent the felspar fragments of the original greywacké. Again we note other subangular fragments, which consist sometimes of fair-sized quartz-granules together with a little dark-coloured mica, or perhaps an iron oxide, sometimes of smaller quartz-granules and of brown mica. Comparing these with specimens of fine-grained earthy sandstones, silts, and shales, affected by contact metamorphism, which we have obtained from other localities,* we find a perfect correspondence in composition and structure, and have no hesitation in regarding them as representatives of bits of arenaceous and argillaceous rock, once present in the original greywacké. Fragments of granitoid or of volcanic rock have not been positively identified in the present set of slides, though very probably the former, at any rate, would be found, if a larger series was prepared. The matrix of the original rock, once a silt of variable composition, is now replaced by quartz and mica (generally brown), with sometimes a considerable proportion of a pyroxenic mineral, which appears to substitute itself for the brown mica.

In conclusion, a few remarks may be made upon the specimens. The first group (see above, p. 194) was taken from near the granite mass. All the specimens have a hard, strong appearance; some show a slight mineral banding, but there is no well-marked foliation; thus the texture of the rock at the first glance reminds one more of a rather micaceous band in those granulitic rocks which are generally

* Bonney, 'Quart. Journ. Geol. Soc.,' vol. 44, p. 11.

found to be of Archæan age, and of which the origin is still doubtful, than of a normal mica-schist. They exhibit that peculiar purplish-brown or purplish-grey tint which is so common in the quartz-mica rocks resulting from contact metamorphism, and is doubtless due to the peculiar tint of the micaceous constituent.

Of the first three specimens, 406, 407, 408,* taken from the same locality, about 12 yards from the granite, we need only remark that one exhibits, macroscopically and microscopically, a slight mineral banding, indicated by variations in the proportions of the quartz, mica, and a brownish earthy-looking dust, which are the chief constituents of the rock. To this banding the flakes of mica tend to lie roughly at right angles. In the other two specimens, fragments of quartz and of altered felspar can be detected. In one the latter is abundant, and the frequency of an earthy dust, and of a mixture of microcrystalline quartz with tiny flakelets of mica or a kaolinitic mineral, renders it probable that the original rock was a rather felspathic grit. A vein of calcite is present in the slide. The other specimen possibly contains a little andalusite.

The next specimen (409) exhibits fragments rather distinctly, some being about 0·05″ in diameter. In those of felspar traces of twinning can be detected, showing the mineral to be either a plagioclase twinned on the pericline type or a microcline. The matrix is more free from "dust" than in the former specimens, so that the rock has a more distinctly holocrystalline aspect. A few grains of epidote are probably present.

The next two specimens (410, 411) were collected about 40 yards from the nearest visible granite. Both assume a tinge of green in weathering. One differs little from some of those already described; the other is macroscopically rather distinctly "spotted;" both exhibit light specks such as might indicate a felspar, and elongated darkish spots or streaks lying roughly parallel. Hornblende is rather abundant, associated with or replacing the brown mica; also a few grains of pyrite. There are fragments as above described, one or two reaching 0·04″ in diameter; the presence also of rock fragments is suggested, but the evidence is inconclusive. From the condition of the matrix one would infer a rather earthy condition for the original grit.

The next specimen (414) is from a junction, and a vein of light coloured, finely granular, non-micaceous granite, about 1½″ thick, is present in the hand specimen. The altered rock is rather more coarsely crystalline in aspect than the others, white mica is present in fair-sized flakes, and there are many irregular blackish spots, like stains, about 0·14″ diameter. Microscopic examination shows that a fragmental structure is still marked, and that bits of rock are present.

* These numbers are painted on the specimens.

Much of this must once have been a more or less sandy argillite. One or two, however, suggest the former presence of a fine-grained quartz-felspar rock, probably a micro-granite, the felspar of which has been replaced by a minute fibrous mineral, the fibrillæ of which exhibit bright tints when rotated between crossed Nicols, and give straight extinction; probably it is sillimanite. White mica is present in rather considerable quantity, in thickish flakes up to about 0·04″ in longer diameter. Sillimanite is also present in the matrix, and in parts of the slide a clear mineral, varying in colour from orange to rich burnt-sienna brown, forms a kind of base to the small round grains of quartz and flakes of brown mica. This material has but a weak depolarising power. It exhibits an aggregate structure, and appears to be in rather close relation with the above-named fibrous mineral. It may be only a stained variety of the same, or possibly an alumina-subsilicate nearly related to staurolite.*

Proceeding onwards, the next specimen (415), taken about 10 yards from the junction, is macroscopically very distinctly speckled, and under the microscope indicates very clearly the presence of fragments. These are quartz and felspar as usual, an altered argillite, and in one case an impure fine-grained sandstone or quartzose silt. In the matrix is a considerable amount of hornblende.

The next specimen (416), collected about 200 yards north of the last, contains no hornblende, and is less definitely fragmental. The flakes of brown mica are not seldom about 0·01″ long, and occasionally rather more. They indicate a slight foliation, and are frequently crowded round the quartz grains, the latter being unusually clear and free from enclosures.

The next two specimens (417, 418) illustrate a rock with bands of brownish and greenish-grey. Fragments are present, as above described, some evidently have been fine-grained sandstones, a few probably an argillite. The amount of these and the changes in the matrix from the ordinary quartz-mica rock to layers where a pyroxenic mineral replaces more or less the mica, indicate that the original rock was composed of stratulæ of different nature, and a slight foliation may be noted parallel with the original bedding. Some of the pyroxenic mineral is hornblende, but in parts of the slide an augite is abundant, as described above.

The last specimen (419) from this locality gives less distinct indications of original fragments, and differs only from the ordinary quartz-mica rocks in being rather more definitely foliated.

The next two specimens (420, 421) were collected at Newton Stewart Road, about ½ mile from the granite, from beds which struck

* A rather similar mineral occurs in a junction specimen from Sinen Gill, Skiddaw, but here its structure is more uniform and its influence on polarised light more marked. This very probably is staurolite.

towards it. Both are slightly foliated. One (not sliced) is the ordinary finely granular quartz-mica rock, but having some thin pale-green laminæ; the other, in which the greener bands predominate, occurs about 5 yards above it. In the slide there is very little brown mica, the pyroxenic constituent predominating. Most of this is hornblende, but some may be the augite already described. There is a little of what seems to be dirty calcite, and a few grains which may be either an impure epidote or possibly sphene. Rock fragments cannot be distinguished; probably the rock was originally a rather fine-grained earthy grit. Occasionally the pyroxenic minerals lie with their longer axes at a high angle with the general foliation of the rock.

The remaining specimens (422, 423) are from near a bridge over the Ken, about 1½ miles from the nearest visible granite, and on the line of strike of the last named. The macroscopic aspect of the one is perplexing. In some respects it resembles an ordinary, fairly coarse mica-schist, rather crushed out and cleaved; in some a schistose micaceous grit; while in some it recalls the specimens above described. In any case it evidently has been affected by pressure. Microscopic examination discloses a distinctly fragmental structure, but in addition some mineral change. The rock mainly consists of grains and granules of quartz, and of flakes of mica of every tint, from almost white to olive-brown, and of variable dichroism, with earthy grains and specks indicating the presence of iron oxides and of an aluminous mineral; a few better preserved fragments are also present, which are almost certainly more or less altered felspar.* There is a little recognisable epidote, and one or two small zircons. The quartz is no doubt in part secondary, but the larger grains have a distinctly fragmental aspect; these occasionally measure about 0·03″ in their longer diameter, but commonly not more than 0·01″. Sometimes the boundary is fairly sharply defined, but generally (especially at the ends of the grains, that is, in the direction of the foliation planes) small mica flakes seem to pierce the edge. This appears to indicate that there has been some secondary enlargement. The grains are occasionally composite, but generally homogeneous; they are rather free from cavities, bubbles or other enclosures, but now and then they enclose microliths, especially of pale-coloured mica. The latter mineral, in like way, is sometimes collected in elongated patches, suggestive of a flaky fragment, sometimes scattered in the ground-mass. The single flakes occur up to about 0·005″ in length, but usually are between 0·001″ and 0·003″. As described by one

* One of these, which may be either a felspar or one of the andalusites, exhibits roughly parallel lines of dark belonitic enclosures, as described by Professor Bonney ('Quart. Journ. Geol. Soc.,' vol. 45, p. 100). These make an angle of about 30° with the general direction of foliation, and are clearly antecedent to it.

of us in the case of the Obermittweida conglomerate,* the mica appears to be partly original, partly secondary; indeed, repetition may be saved by referring to the published description of the grit from the inner part of the fold at that place, merely stating that in it secondary changes appear to be rather more pronounced than in the present specimen.

The other specimen is a homogeneous, compact, very fissile rock, like a schistose slate or phyllite. Microscopic examination shows that it consists of a micaceous mineral and quartz, with occasional granules of epidote (rare), of an impure kaolinitic mineral, and flakes or perhaps rods of iron oxide. In most parts of the slide these are the chief constituents. The flakes of the first-named mineral occasionally, in the more quartzose parts, are about 0·0025″ long, but generally less. It varies from almost colourless to a pale olive tint, but sometimes is slightly brown. It often resembles a mineral common in some of the oldest green slates, and some of it is probably a chlorite. The associated quartz is generally very minute, but rather larger grains of distinctly fragmental aspects, up to about 0·004″ in diameter, occur, especially in certain layers, and there are others consisting mainly of quartz. It is, however, possible that some of these may be veins, indeed, one of the most quartzose certainly is, but others are hardly less certainly true stratulæ, indicative of former bedding. The sharp flexures in these, the "rucking" of the layers of mica, and the development in parts of the slide of an incipient strain-slip cleavage (*ausweichungs-clivage*) indicate that the rock, subsequent to partial mineralisation, underwent great pressure. In short, macroscopically and microscopically, it bears considerable resemblance to those "satiny" slates or phyllites, the alteration of which is, to a very large extent, due to pressure. At the same time, when we examine the individual plates of mica and the structure of the folds with a high power, we are led to think that some subsequent modifications have taken place. As has been described in the paper already mentioned, each individual flake of mica appears as if the process of formation had been completed *in situ*, or that in some way or other it had been enabled to "right itself" after the distortion which is usually produced by these great earth-movements. So also the layers of mica, as it were, slightly bristle with the projecting ends of mica flakes; at the sharpest part of a fold they have lost the "strained" look, like a rope beginning to part, and the incipient plane of strain-slip cleavage is often "soldered up." The changes have taken place (though to a much less extent), which are described in the paper by one of us on the effect of contact metamorphism on phyllites at Morlaix.† Probably we shall not be wrong in ascribing them to the elevation of temperature, connected

* 'Quart. Journ. Geol. Soc.,' vol. 44, p. 29.

† 'Quart. Journ. Geol. Soc.,' vol. 44, p. 12.

with the granite intrusion above described, though in the present case the greater distance from the granite would make this elevation a less important agent of change.

The specimens which have been described indicate that from ordinary muds and sandy silts, quartz-mica (and in some cases quartz-pyroxene) rocks may be developed by contact metamorphism, and that the differences now to be observed in the mineral composition are due to differences in the original sediments of which the mass was composed. In some cases the rock has become thoroughly crystalline, in others the process is less complete, and a fair quantity of the original dust, possibly in the form of kaolin, still remains. If fragments of larger size have been present, these, though modified like the matrix, can still be recognised. Some of these rocks are no less crystalline than certain of the less coarsely crystalline mica-schists, and occasionally exhibit a foliation. From the latter, however, they can be distinguished by a practised eye. They are fair imitations of some of the indubitably Archæan quartzose mica-schists, but only imitations. Heat has been the main agent of metamorphism in the case of the rocks just described, though probably water was present, and considerable pressure may also have been exercised, which in one case seems to have produced an earlier alteration. Where the original constituents have differed considerably in size, a record of this structure is still retained. Had the elevated temperature been maintained for a longer time, molecular movements among the constituents might have rendered this structure more indistinct, but there is nothing to warrant the supposition that they could have obliterated the distinction between stratulæ of moderate thickness. These specimens then appear to justify us in asserting a sedimentary origin for certain crystalline schists (micaceous, quartzose, &c.), in referring their mineral bands to a stratification of the materials, and in supposing their alteration due to their having been kept at a comparatively high temperature for a considerable period.

III. "On some Variations of *Cardium edule*, apparently correlated to the Conditions of Life." By WILLIAM BATESON, M.A., Fellow of St. John's College, Cambridge, and Balfour Student in the University. Communicated by ADAM SEDGWICK, F.R.S. Received May 13, 1889.

(Abstract.)

In 1886 and 1887 I made a journey to some of the lakes of Western Central Asia for the purpose of making observations on their fauna. As the waters of these lakes are of very various composition, being salt, alkaline, bitter or fresh, in different degrees, I looked forward

to an opportunity of investigating the question whether these diverse environmental conditions produce any correlated changes in the structure of the animals exposed to them. The collections made with this object consist chiefly of Crustacea, of which an account will appear hereafter. The shells forming the subject of the present paper were collected in the district of the Aral Sea and in Egypt.

As is well known, the Aral Sea formerly covered a larger area than it does at the present time. The limits of this area have not been determined, but it has been vaguely suggested that the Aral and Caspian Seas together covered a large part of the steppes of Western Central Asia, probably connecting with the Arctic Ocean, and that the sea thus formed gradually receded until the Aral and Caspian Seas alone remained in their present form. In the course of my journey I visited the valley of the Irghiz and Turgai Rivers and Lake Tschalkar into which they lead; the north and north-west shores of the Aral Sea; the valley of the Shu and Tele Kul Tata into which it leads; also Lake Balkhash, and I nowhere found any direct evidence which could at all support the view that the Aral Sea had gradually receded from a very greatly extended area. On the contrary, all trace of the previous presence of the sea disappeared everywhere abruptly at a level about 15 feet higher than the present level of the Aral Sea. In all places which would be covered if the sea were to rise about 15 feet, shells of the cockle of the Aral Sea (*Cardium edule, var. rusticum*) were found deposited in great quantities, but they always ceased at a definite horizon on ascending. In some places the shore of the Aral Sea is formed of vertical cliffs composed of horizontal beds containing fossils of the age of the London clay of England,* while elsewhere the beach consists of sandy flats extending inland for many miles. Both in the steep places and on the sands the deposits of cockles cease suddenly as described. This seems to be conclusive evidence that the Aral Sea has at all events not *continuously* receded from a very much larger area than that which it at present occupies.

On the north shore of the gulf, Sary Cheganak, a considerable strip of low-lying country has been uncovered. The average width of this shell-covered region is about 3 miles. The post road to Kazalinsk and Tashkend crosses it from north to south. At the post station, Ak Jalpas, is a narrow, dry channel, like the bed of a river. This channel passes up from the Aral Sea, and leads to two large basins which were formerly connected with the sea itself, but which are now dry. The southern basin is called Shumish Kul, and the northern is known as Jaksi Klich. In the course of the channel by which Jaksi Klich was joined to the Aral Sea is a smaller basin,

* These fossils were kindly examined for me by Mr. T. Roberts, of St. John's College, Cambridge, and Mr. Keeping.

Jaman Klich. When the level of the Aral Sea fell, each of these three basins became isolated as a separate salt lake containing a sample of the fauna of the Aral Sea. All three lakes subsequently dried up, and during this process the water must have become salter and salter until only a bed of salt remained.

Shumish Kul is about 8 miles long. The western shore is bounded by high limestone hills, on the foot of which definite terraces are marked by the action of the water as the level of the lake gradually sank during the process of drying up. On these terraces are great numbers of shells of the cockle of the Aral Sea (*Cardium edule, var. rusticum*), being for the most part paired shells with their ligaments preserved, fixed on the oral faces in the crust of oxide of iron and mud, which was formed when the waters of the lake covered them. A series of specimens, therefore, taken from these terraces gives examples of the shells as they were at each stage in the progressive desiccation of the lake, and shows the changes which they underwent as the lake became salter and salter. The principal terraces are seven in number, and the total difference in level between the top and the bottom is about 60 feet.

On comparing the shells found on successive terraces from above downwards, the chief variations noticeable in them are as follows:—

(1.) *Diminution in the Thickness of the Shells.*—This is first apparent in the shells of the third terrace. It proceeds to such an extent that the shells of the lowest terrace are almost horny and semi-transparent. The change in the thickness of the shells is most clearly brought out by the table giving the comparative weights of the shells from different terraces.

(2.) *The Size of the Beak is greatly reduced.*

(3.) *The Shells become highly coloured.*—This change and (1) occur almost uniformly. The shells of each terrace vary very little among themselves in texture, thickness, and colour.

(4.) *The Grooves between the Ribs appear on the inside of the Shell as Ridges with rectangular Faces.*—This change first affects only the ribs behind the 8th and 10th, but on the lowest terraces all the ribs are so affected.

(5.) *On the lowest Terrace the Shells diminished greatly in absolute Size.*

(6.) *The Length of Shells in Proportion to their Breadth increases.*—I use the term "length" to mean the greatest antero-posterior dimension, and the term "breadth" to mean the dorso-ventral measurement at right angles to the length, passing in right valves across the point of the posterior tooth, and in left valves across the depression into which the posterior tooth of the right valve fits. I have found by careful testing that these measurements can be relied on to 0·5 mm.

In the course of these measurements it appeared that—

(*a*.) The change in proportion does not occur in all shells, nor to an equal degree in those in which it is found.

(*b*.) This variation is far more marked in shells of greater absolute size, making it necessary that samples of shells of nearly the same size must be taken for comparison.

(*c*.) This variation occurred slightly in the shells of the 2nd terrace, increasingly in those of the 3rd and 4th, reaching a point in the 5th terrace which is practically not afterwards exceeded, even in shells found as much as 30 feet lower, though the changes in texture, &c., had greatly progressed in these latter. (See tables.)

In this lake shells of *Dreissena polymorpha* were found on the level of the 3rd terrace, and shells of *Hydrobia ulvæ* on most of the terraces, *which did not differ from those of the Aral Sea.*

Jaksi Klich is the largest superficially of the three dry lakes containing cockles. Its length is about 10 miles and its breadth 3 miles. It differs from Shumish Kul in being comparatively shallow, the former having been about 60 feet deep before the separation from the Aral Sea, while the latter cannot have been more than 15—20 feet deep. There is no distinct series of terraces on its banks, but the shells occur in two chief deposits, an upper and a lower deposit. The outer deposit marks the original high level of the water, and the other forms a band of shells round the salt which now fills the bottom of the lake.

Generally speaking, the shells of the lower deposit show the same variations in texture, colour, shape, &c., when compared with those of the upper deposit, as were found in the case of the shells of Shumish Kul. But while the shells of the highest terrace at Shumish Kul were practically the same as those now living in the Aral Sea, those of the upper deposit at Jaksi Klich differ in some particulars, probably in connexion with the fact that Jaksi Klich was always only a shallow lagoon, while Shumish Kul was a deep lake.

The variation in the proportion of length to breadth reaches a greater development in the shells of the inner deposit at Jaksi Klich than in any other shells examined (see tables), excepting those of the fresh water lake, Ramleh, No. 2.

Jaman Klich is a small, independent lake-bed, about half a mile in diameter and about 15—20 feet deep. Its shells show the same variations as those of Jaksi Klich. (See tables.)

On the flats between Shumish Kul and the other two lakes are a considerable number of *very large* shells of *Cardium edule*. These shells have special characters, and perhaps form a distinct variety. Some of them were found at the bottom of Jaksi Klich and Jaman Klich, also in a small dry lagoon, lately separated from the Aral Sea, near Alta Kuduk. As will be hereafter mentioned, similar shells were found deposited in great numbers beneath the surface-soil at

Abu Kir, in Egypt. Shells of this type would appear to be in some manner associated with life in lagoons opening to a sea, as all the localities in which they occurred were of this type. None were found at Shumish Kul or in the Aral Sea itself.

Cardium edule from Lagoons in Egypt.

I collected shells of *C. edule* from the district of Mareotis and Abu Kir in order to compare them with those of the Aral Sea.

Abu Kir has now been pumped dry. In 1888 it was a shallow salt lake, having an area of about 20 square miles. In April, 1888, the specific gravity of the water was 1·05. No living shells were found in it, but its shores were covered with vast quantities of *thin, elongated, highly-coloured shells*, closely resembling those of Jaman Klich. These shells were plainly those which had last lived in the lagoon, and it may be supposed that they lived in it under conditions not greatly different from those now prevailing.

Mareotis.—This is a closed lake lying about 8 feet below the surface of the Mediterranean. At the time of my visit, in April, 1888, the density of the water was about the same as that of the Mediterranean. It is stated to be brackish at high Nile owing to the infiltration of fresh water, though in summer it probably becomes salter than the sea. On its shores I found quantities of shells of *Cardium edule.* There are, apparently, none now living in Mareotis. The absence of living animals may be due to the annual changes which the quality of the water of the lake undergoes, but it is more likely that they have all been exterminated by some of the engineering operations which have at various times been made by different Governments. The shells found on the shore had definite characters. They were elongated shells, moderately thin in texture, having the anterior 6—10 ribs yellowish-white in colour, and the portion 7—12 bluish or chocolate-coloured. The inside of the shells is much ribbed; the posterior part is generally chocolate-coloured, and sometimes this colour extends to the whole interior of the shell. (For particulars of texture and shape, see tables.) The peculiarities in colour and shape of these shells are so great that they could not be mistaken for those of any other locality.

Ramleh Lake No. 1.—By the formation of the Mahmudiyeh Canal (1819) a small piece of water was cut off from Mareotis near Sidi Gaber Station. This lake is about a mile in diameter. Its water is now fresh, and is kept so by the waste water from the irrigations which flows into it. It is about 10—12 feet deep in the middle. Many dead cockle-shells were found in it, but no live ones. These shells have quite definite characters, being very thick and coarse in texture, with 14—16 anterior ribs white, and 3—6 posterior ribs

chocolate colour. The shells are very long in proportion to their breadth. (See tables.)

Ramleh Lakes No. 2 *and No.* 3.—By the construction of the railway from Alexandria to Cairo another portion of Mareotis has been cut off by an embankment, and the lake thus formed was again divided into two by the second embankment lately made to connect the Cairo Railway with the Ramleh line. In this way two lakes have been formed—an eastern (No. 2) and a western (No. 3). Both these lakes are fresh owing to irrigation-waters. In No. 3 there are no shells of *Cardium* at all, but in No. 2 I found quantities of living specimens. These fresh-water cockles were in texture like the shells found in Ramleh Lake No. 1, but the colour and other features were different. The colour of the outside of the shells is almost uniformly yellowish-white, but on the inside the region of the posterior 3—6 ribs is chocolate colour. The rest of the inside of the shell has the same bright white colour which characterises those of Ramleh Lake No. 1. The proportion of length to breadth in these shells is very great. Another character of these fresh-water shells is the frequent occurrence of specimens with the free ventral margins of the valves bent inwards.

Sub-fossil Shells.—At Mandara and elsewhere I found considerable deposits of very large, thick shells, like those found occasionally at Jaksi Klich in the Aral Sea district. Probably those shells were deposited at the time when Abu Kir and Mareotis formed one or more large lagoons in communication with the open sea.

Recapitulation.

The most important feature of these observations lies in the fact that *the shells of each sample, whether it be from a separate lake or only from a particular level, have special characters, and are more like to each other than to the shells of one of the other lakes or of another level.* The next feature of importance is the fact that in the four independent cases, Shumish Kul, Jaksi Klich, Jaman Klich, and the Egyptian lagoon Abu Kir, *the shells which have lived under similar conditions*, i.e., *in very salt water, resemble each other*, having the characters of thinness, light colour, small beaks, ribbing on the inside of the shell, and great relative length. *Similarly the shells from the two isolated and independent fresh-water lakes at Ramleh also present similar characters, viz., thickness, similar texture, and shape.* It may be remarked that the resemblance between the cockle-shells from an Asiatic lagoon and those from Abu Kir becomes still more striking when it is remembered that their immediate ancestry is very different. For the Asiatic shells had been living for many generations in the brackish waters of the Aral Sea, and had already

become a well-marked variety before being subjected to the new conditions; while those which are found in Abu Kir must clearly be the immediate descendants of animals of the type found in the Mediterranean.

Though the subject cannot be adequately discussed in an abstract, it may be suggested that in so far as any variation (as, for example, that of texture) occurs universally among the shells of a given sample, it may be legitimately supposed that they are correlated to the conditions under which they lived.

Instances in which it is possible to actually trace the history of variation under natural conditions are so rare that these observations of phenomena otherwise unimportant have an increased value. The opportunity given by the terraces of Shumish Kul for the comparison of several distinct stages in the origin of a natural variation appears to be almost unique.

Table showing the Average Ratio of Length to Breadth in Shells from the various Localities.

In each case the average was taken in thirty individuals. It is impossible in an abstract to give the particulars of the measurements; these appear in the fuller account. This table gives a summary of the results. The extremes of length of the shells measured are given in millimetres and the average breadths are given in terms of the length, which is taken as 1.

Locality.	Level.	Smaller samples.		Larger samples.	
		Extremes of length in mm.	Average breadth.	Extremes of length in mm.	Average breadth.
Shore of Aral Sea	..	22—18·5	0·761		
Shumish Kul ...	1st terrace	21—17	0·799		
Ditto	2nd terrace	21—17	0·782	26—19	0·770
Ditto	3rd terrace	22—18	0·751		
Ditto	4th terrace	21—16	0·735	26—18	0·730
Ditto	5th terrace	21—16	0·743	27—21	0·731
Ditto	7th terrace	21—16	0·725		
Jaksi Klich	Upper deposit	22—17	0·740		
Ditto	Lower deposit	25·5—19	0·682	30—25·5	0·660
Jaman Klich....	Lower deposit	24—16	0·726		
Shore of Mareotis	..	..	..	27—20	0·680
Ramleh Lake No. 2	(Fresh water)	21—17	0·665	29—16·5	0·657
Shore of Abu Kir	..	24—19·5	0·738		

Table showing the comparative Weight of Shells of similar Size.

Locality.	Level.	Extremes of length in mm.	Average length of 20 specimens.	Total weight in grams of 20 specimens.
Shore of Aral Sea	..	21—17	19·2	13·3
Shumish Kul	1st terrace	21—17	19·1	14·1
Ditto.................	2nd terrace	21—17	19·4	14·5
Ditto.................	4th terrace	21—17	19·2	6·5
Ditto.................	5th terrace	21—17	18·9	6·1
Ditto.................	7th terrace	21—17	19·7	4·6
Shore of Abu Kir	..	21—17	19·0	6·4
Jaksi Klich	Upper deposit	23—19	20·4	7·8
Ditto.................	Lower deposit	23—19	20·4	5·5
Jaman Klich	Lower deposit	21—17	19·2	5·1
Sub-fossil shells at Mandara	..	26—21	23·4	24·2
Shore of Mareotis	..	25—22	23·8	12·0
Ramleh Lake No. 1	(Fresh water)	25—20	21·4	18·3
Ramleh Lake No. 2	(Fresh water)	26—23	24·1	23·6

IV. "On the Occurrence of Skatole in the Vegetable Kingdom." By WYNDHAM R. DUNSTAN, M.A., F.C.S., Professor of Chemistry to, and Director of the Research Laboratory of, the Pharmaceutical Society of Great Britain. Communicated by W. T. THISELTON DYER, C.M.G., M.A., F.R.S. Received May 23, 1889.

Skatole is the name given by Brieger ('Deutsch. Chem. Gesell. Ber.,' vol. 10, p. 1027; 'Journ. für Prakt. Chem.' [2], vol. 17, p. 129) to a substance he obtained in 1887 from human excrement (τὸ σκώρ, σκατός) which possessed the disgusting odour of the fæces. Nencki ('Journ. für Prakt. Chem.' [2], vol. 17, p. 98) soon afterwards recognised the same substance among the products of the decomposition of albumen by fused potash. Secretan ('Deutsch. Chem. Gesell. Ber.,' vol. 10, p. 1031) isolated skatole from the products of the putrefaction of albumen. Later, Salkowski ('Deutsch. Chem. Gesell. Ber.,' vol. 12, p. 651) separated it from the putrefaction products of flesh and afterwards from those of various forms of animal proteid ('Zeits. für Physiol. Chem.,' vol. 8, p. 417). Tappenheimer ('Deutsch. Chem. Gesell. Ber.,' vol. 14, p. 2382) has found skatole in the intestines of several herbivorous animals, and recently Stoehr ('Deutsch. Chem. Gesell. Ber.,' vol. 20, p. 1108) has obtained it, along with indole and other substances, by distilling strychnine

with lime. Nencki ('Journ. für Prakt. Chem.,' vol. 20, 1879, p. 466) by a series of analyses of skatole and its picrate, showed that the composition of the substance is expressed by the formula C_9H_9N, and that it might be regarded as the methyl-derivative of indole (C_8H_7N), a compound which is associated with skatole in the fæces, and which is also produced during the putrefaction of albumen. In 1880 von Baeyer ('Deutsch. Chem. Gesell. Ber.,' vol. 13, p. 2340) described a method of preparing skatole, together with indole, from indigo, which was reduced with tin and hydrochloric acid, and the resulting reduction product distilled with powdered zinc. Three years later, in 1883, Otto Fischer and German achieved a synthesis of skatole ('Deutsch. Chem. Gesell. Ber.,' vol. 16, p. 710) by heating glycerol with a compound of zinc chloride and aniline, $C_6H_7N + C_3H_8O_3 = C_9H_9N + H_2O$. Fileti ('Gazz. Chim. Ital.,' vol. 13, p. 350) in the same year prepared skatole by distilling the barium salt of orthonitrocumic acid with powdered zinc, $C_9H_{11}NO_4 + 3Zn = 3ZnO + H_2O + C_9H_9N$, and he inferred from this synthesis that skatole is Pr-3 methyl indole, an inference which has since been fully substantiated.

Quite recently a most interesting synthesis of skatole has been made by Emil Fischer ('Liebig's Annalen,' vol. 236, 1886, p. 137) from propylidene phenylhydrazide. This compound when heated with zinc chloride yields ammonia and Pr-3 methyl indole (skatole),

$$Ph.N_2H{:}CH.CH_2CH_3 = NH_3 + C_6H_4 {<}\begin{smallmatrix} \overset{\displaystyle C(CH_3)}{\overset{|}{-C}} \\ NH \end{smallmatrix}{>} CH.$$

Neither skatole nor indole has hitherto been observed to occur in plants. They have appeared to be characteristic products of the bacterial resolution of animal proteid.

Among the many plants collected by the late Daniel Hanbury, F.R.S., and now deposited in the Museum of the Pharmaceutical Society, there was a small specimen of wood to which the Curator, Mr. E. M. Holmes, drew my attention on account of it intense odour, which recalled that of α-naphthylamine. Since α-naphthylamine has not been noticed as a plant constituent I determined to examine the specimen, which was labelled *Celtis reticulosa*, Java. It was evidently the wood of a large tropical tree.

In order to be well assured of the identity of the specimen I applied to Mr. Thiselton Dyer for information about this plant, and I am greatly indebted to him for the trouble he has taken in ascertaining what is known on the subject. Mr. Dyer informs me that "*Celtis reticulosa* was the name given by Miquel to a tree growing in Java. Planchon subsequently identified the species with *Celtis cinnamomea* of Lindley (De Candolle's 'Prodromus,' vol. 17, p. 181), which occurs throughout Eastern India and Ceylon. Thwaites regards the Ceylon

form as a distinct species, under the name of *Celtis dysodoxylon* ('Enum. of Ceylon Plants,' p. 267). But botanists generally sink it with the Java species in *Celtis cinnamomea*. Thwaites says: 'The freshly cut timber of the tree possesses a powerful and very disgusting odour.' I have not come across any other notices of this singular property. But the evidence, though indirect, goes to show that your wood is correctly named and what it professes to be. *Celtis* belongs to the *Urticaceæ*."

The total quantity of the wood I was able to obtain amounted to rather less than 200 grams.

A small quantity of the finely powdered wood was moistened with water and distilled with steam. The first fractions of the distillate contained white particles which dissolved in the larger quantity of water that was subsequently condensed. The distillate was examined for naphthylamine, but none could be detected.

On submitting to steam distillation a larger quantity of the powdered wood, and extracting the aqueous distillate with light petroleum, a substance possessing an intolerable odour of fæces was obtained. It crystallised from water in colourless scaly crystals, which were dissolved by ether, alcohol, and benzene. The aqueous solution was not precipitated by a saturated solution of picric acid until the liquid had been strongly acidified with hydrochloric acid, when a dark red precipitate appeared. The dilute aqueous solution was not coloured or otherwise affected by the addition of fuming nitric acid or by a mixture of sodium nitrite and sulphuric acid, neither did it colour a pine shaving moistened with hydrochloric acid. By warming with hydrochloric acid the aqueous solution was coloured cherry-red. By these reactions the absence of indole was conclusively proved, but it seemed probable that the substance might be an indole-derivative.

The picrate was obtained by precipitating the aqueous solution (prepared from the ethereal extract of the distillate) strongly acidified with hydrochloric acid, with a saturated solution of picric acid. The dark-red precipitate was collected and distilled with dilute ammonia. The recovered substance was again converted into the picrate by a repetition of the process described above, and this was collected, washed with cold water, and dried over sulphuric acid. The dark-red needles of the picrate melted with some decomposition between 159—161° C. In a portion of this salt the nitrogen was determined by Dumas' method, the gas being collected through the limb of a Sprengel pump. [Weight of picrate taken 0·0258 gram; corrected volume of nitrogen obtained 3·2 c.c. Percentage of nitrogen in the picrate 15·5. Calculated for $C_9H_9N.C_6H_2(NO_2)_3OH$, 15·5 per cent. of nitrogen.]

It was thus proved that the substance possesses the composition of

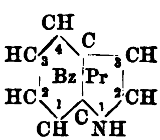
methyl indole. It remained to determine whether it corresponded with any of the known isomeric methyl indoles. These are Pr-1 methyl indole, which is liquid above −20° C.; Pr-2 methyl indole (methyl ketole), which melts at 59° C.; Pr-3 methyl indole (skatole), which melts at 93—94° C. (Nencki), 95° C. (E. Fischer), and Bz-3 methyl indole, which melts at 58° C.*

A crystalline specimen of the methyl indole was prepared from the picrate by distilling it with dilute ammonia. The distillate was extracted with ether, and the residue left by the evaporation of the ethereal solution was crystallised from water. These crystals were observed to melt at 93·5° C., and were, therefore, identical with Pr-3 methyl indole, the skatole of Brieger. To completely confirm this conclusion a synthesis of skatole was made from propylidene-phenyl-hydrazide, and the properties of the synthetical skatole were found to be identical with those of the skatole from *Celtis reticulosa*. It was not possible with so small a quantity of material to estimate the amount of skatole contained in the wood, but it is extremely small, and is certainly considerably less than 1 per cent.

Conflicting statements exist with reference to the odour of skatole. The skatole from *Celtis reticulosa* undoubtedly possesses a strong fæcal smell. Von Baeyer has stated that when pure, skatole has no fæcal odour, while Brieger, Emil Fischer, and others assert that it has. Some curious facts were observed during the course of this investigation which may throw some light on the cause of this divergence of opinion. It was noticed that the fæcal smell is most marked when the substance is present in minute quantities. When larger quantities of the substance are smelt the odour appears to be penetrating, but more aromatic than fæcal. It also seems that after repeatedly smelling the substance the nose becomes insensitive to the effect of minute quantities and fails to recognize the fæcal odour.

It may be useful to refer here to the delicacy of the various reactions for skatole and indole, since it is often important to recognise the presence of very small quantities of these substances in pathological fluids. The formation of the white insoluble nitroso-derivative from skatole by the action of nitrous acid cannot be

* The nomenclature followed is that proposed by Emil Fischer, the isomerides in which the substitution occurs in the pyrrole nucleus of indole being distinguished as Pr-derivatives, and those in which substitution takes place in the benzene nucleus as Bz-derivatives. The hydrogen atoms of the two nuclei are numbered in accordance with the following plan—



recognised in very dilute solutions, but the production, under the same conditions, of a reddish colour or precipitate is a sensitive test for the presence of indole.

By far the most delicate reaction for skatole consists in the development of a cherry-red colour when an aqueous solution is warmed with a few drops of strong hydrochloric acid. This reaction has been obtained with very dilute solutions. In moderately dilute solutions, strongly acidified with hydrochloric acid, a saturated solution of picric acid produces a red precipitate of skatole picrate.

The occurrence of skatole in the vegetable kingdom is of especial interest, and it is remarkable that the substance should make its appearance, or, at any rate, that it should accumulate, at a late period in the growth of the tree. The absence of indole is also significant. Sometimes skatole, without indole, is observed to result from the putrefaction of animal proteid, but usually both indole and skatole are formed. The occasional absence of indole, as well as the inconstancy of the relation between the quantities of indole and skatole formed during proteid putrefaction, has led Salkowski ('Zeits. für Physiol. Chem.,' vol. 8, p. 417) to regard the formation of each of these substances as due to the intervention of a special ferment, which he has called the indole ferment and the skatole ferment respectively. It must be remembered, however, that indole is less stable than skatole, and would be less likely to survive the effect of the various chemical changes which are proceeding during the later stages of putrefaction. Hoppe-Seyler ('Zeits. für Physiol. Chem.,' vol. 8, p. 214) has shown that if oxygen is freely supplied to a liquid in which proteid putrefaction is taking place, neither indole nor skatole is formed. The superior stability of skatole may possibly explain its survival in the plant. It would be interesting to determine whether indole is present in *Celtis reticulosa* at an earlier stage of its growth, as, for example, at the period when the skatole first makes its appearance.

It would also be of the highest biological and chemical interest to discover if possible the exact source of the skatole in this plant, to determine whether the vegetable proteid of *Celtis reticulosa* can be made to yield skatole by the methods which are known to lead to its production from animal proteid, or whether the skatole has been formed from some intermediate substance, as, for example, an amidated organic acid. The synthetical production of skatole from nitrocumic acid furnishes us with a clue to one possible mode of formation in the plant.

This investigation has been conducted in the Research Laboratory of the Pharmaceutical Society, and my thanks are due to Mr. W. A. Salter for the assistance he has given me with some of the experiments which are recorded in this paper.

V. "The Conditions of the Reaction between Copper and Nitric Acid." By V. H. VELEY, M.A., University Museum, Oxford. Communicated by Professor ODLING, F.R.S. Received May 13, 1889.

Introduction.

About fifty years ago De La Rive* observed that pure metallic zinc but slowly enters into reaction with dilute sulphuric acid; shortly afterwards Faraday† confirmed this observation in the case of amalgamated zinc. In the course of some investigations on the equivalent of this metal, Ramsay and Reynolds‡ failed to obtain any hydrogen from sulphuric acid and samples of zinc which they had purified by every possible precaution. Thus the evolution of hydrogen from zinc and sulphuric acid depends initially upon the presence of some third substance, be it an impurity of the acid or of the metal.

Similarly Russell§ has also observed that metallic silver is slowly attacked by nitric acid freed from any considerable quantity of nitrous acid, and that the rate with which the change proceeds depends upon the proportion of nitrous acid present.

Reactions between Copper and Nitric Acid.

The changes which take place when metallic copper is dissolved in nitric acid have attracted the attention of a number of investigators; the complexity of these changes and their almost infinite variety produced by slight variations of the conditions, are evidenced by the elaborate researches of Deville,|| Armstrong¶ with Acworth and Divers.** In the present communication I have the honour of laying before the Royal Society a short and preliminary account of some experiments on the conditions necessary for a reaction to take place between metallic copper and nitric acid.

The Methods of Experiment.

By means of a mechanical device, described fully in another paper before the Chemical Society, spheres of the purest electrotype copper procurable were placed on a small glass dish which was kept continually revolved in dilute nitric acid; a fairly uniform current of

* 'Annales de Chimie,' vol. 43, 1830, p. 425.
† 'Experimental Researches,' Series VII, p. 863.
‡ 'Chem. Soc. Journ.,' 1887 (Trans.), p. 854.
§ 'Chem. Soc. Journ.,' 1874, p. 3.
|| 'Compt. Rend.,' 70, pp. 20 and 550.
¶ 'Chem. Soc. Journ.,' 1877, p. 54.
** 'Chem. Soc. Journ.,' 1883, p. 443.

carbonic acid was also passed through the liquid to ensure its perfect agitation, and thus to remove the products of the reaction from the immediate vicinity of the metal.

The nitric acid was made up in considerable quantity at a time by diluting acid of sp. gr. 1·41 with a suitable proportion of water; the specific gravity of the diluted acid was taken by means of a very delicate pyknometer of the form devised by Sprengel, and the amount of free acid determined in the same portion by the ordinary process of acidimetry. The weight of the copper sphere, as also its diameter, was determined before and after each experiment, which lasted for one hour; from the diameters the mean area of metallic surface exposed was calculated; thus the amount dissolved off per unit area could be directly determined.

In some earlier experiments made with a view of ascertaining this amount, it was noticed that when the sphere of copper was introduced into the acid the evolution of gas did not commence at once, but if, other conditions remaining the same, the sphere was introduced into the acid containing in small quantities the products of the reaction of a former experiment, the evolution of gas commenced immediately. Further, the amount of copper dissolved per unit area was less in the first than in the succeeding experiments, after the products of the change had been allowed to accumulate in the acid. This will be rendered more evident by the figures given in the table below, the first two columns of which contain the weights of two copper spheres A and B (placed alternately in the acid) before and after each experiment, the third the differences between these two numbers, the fourth the mean area in square millimetres, and the fifth the amounts dissolved off per unit area expressed as decimilligrams per square millimetre surface, written for the sake of brevity M/A. In this and all succeeding experiments a long-range thermometer was used.

Table I.

Sp. gr. of Acid at 19° compared with Water at the same Temperature = 1·1699. Percentage of free Nitric Acid = 27·53. Temperature 25°.

Weight at commencement.	Weight at conclusion.	Loss.	Mean area.	Value of $\frac{M}{A}$.
4·4153 (A)	4·0768	0·3385	291·95	11·60
4·5823 (B)	4·2049	0·3774	300·05	12·38
4·0768 (A)	3·7378	0·338	275·83	12·25
4·2049 (B)	3·8465	0·3584	283·16	12·65
3·7378 (A)	3·4166	0·3125	261·23	12·37
3·8465 (B)	3·5268	0·3197	266·78	12·10
3·4166 (A)	3·1188	0·2975	246·52	12·07

It will be seen from the above table that the amount dissolved off per unit area in the first experiment is less than that in the succeeding experiments, when the products of the change were present in the acid.

In another series of experiments the nitric acid used in the first operation was rejected, and another portion of the same sample was taken for the second operation; but for those succeeding this latter was used again. It was observed that in the first two experiments the evolution of gas did not commence immediately after the introduction of the sphere into the acid, whereas in all the succeeding experiments this was the case.

Table II.

Sp. gr. of Acid at 19° = 1·1643. Percentage of Free Nitric Acid = 27·02. Temperature 20° C.

Weight at commencement.	Weight at conclusion.	Loss.	Mean area.	Value of $\frac{M}{A}$.
4·762	4·6519	0·1102	313·16	3·52
4·6519	4·5451	0·1068	307·29	3·48
4·5451	4·3823	0·1628	300·41	5·42
4·3823	4·2303	0 1520	293·23	5·20
4·2303	4·0838	0·1475	286·77	5·29
4·0838	3 9456	0·1383	280·15	5·05

It will be manifest from the above table that the values for the amount dissolved off per unit area on the first two experiments are concordant among themselves, as also are those of the succeeding experiments, but that those of the former are lower than those of the latter.

When copper is dissolved in nitric acid, the substance more immediately evident to the senses is cupric or cuprous nitrate or nitrite, or possibly a mixture of some of them; at first I was inclined to the belief that the copper salt was the third substance which induced the reaction between the metal and the nitric acid to start at once. Indeed, at a meeting of the Chemical Society I expressed this opinion in the course of a debate.

It was found subsequently that the dilute nitric acid used in the above experiments contained a small trace of nitrous acid. Accordingly a sample of nitric acid of sp. gr. 1·41 was freed from nitrous acid by passing a rapid stream of air through it at a temperature not exceeding 35° C.; during the process the acid was protected from direct sunlight. If the temperature rose to 40° C., or the acid was

not properly sheltered, nitrous acid was formed by the decomposition of the nitric acid at a rate faster than that at which it was oxidised by the air current. The acid was subsequently diluted with the required quantity of water and preserved in a dark cupboard. When 3—4 c.c. of this diluted acid were mixed with about 100 c.c. of water, no blue colour was produced on addition of starch and potassium iodide solutions, and the faintest possible orange tinge imparted to an aqueous solution of meta-phenylene diamine hydrochloride. The acid was thus free from any considerable trace of nitrous acid. Experiments similar to the above were repeated, but the current of carbonic acid omitted, in order to more precisely fix the conditions.

After introduction of a copper sphere there was no evolution of gas for three minutes, and five minutes after the reaction had set in a considerable quantity of nitrous acid was shown to be present. As regards the production of nitrous from nitric acid under these conditions, Professor Armstrong writes, in a note appended to a paper by Dr. Divers:* "With reference to the formation of N_2O_4 during the dissolution of metals, &c., I some time ago satisfied myself by experiment that it is produced . . . . and there is, I believe, no doubt that, whatever the nature of the reducing agent, be it hydrogen or metal, ... the primary product of the reduction of nitric acid is nitrous acid." This observation, made some time ago, is amply confirmed by the above and succeeding experiments. I would, therefore, merely wish to call attention to the short interval of time which elapses between the commencement of the reaction and the formation in considerable quantities of nitrous acid. Again, when the copper sphere was introduced into the acid containing the copper salt and the nitrous acid, the evolution of gas commenced at once.

In another experiment, in which the copper sphere was previously heated and then allowed to cool in a current of hydrogen to remove any superficial coating of cupric or cuprous oxide, no gas was evolved for 1′ 50″ after the introduction of the sphere into the acid, and 4′ 30″ after the reaction had set in an abundance of nitrous acid was shown to be present.

At this point, Mr. Harcourt, from a recollection of some experiments by the late Sir Benjamin Brodie, suggested to me to place into the acid a substance which should remove the nitrous acid as fast as it might be formed; urea fulfils this requirement, in that it reacts immediately with nitrous acid to form nitrogen and carbonic acid, according to the equation

$$CO(NH_2)_2 + 2HNO_2 = CO_2 + 2N_2 + 3H_2O.$$

Accordingly, 1 gram of urea, dissolved in 1 c.c. of water, was added to a litre of nitric acid (sp. gr. $\frac{16}{15}$ = 1·1662, percentage of

* 'Chem. Soc. Journ.,' 1883 (Trans.), p. 456.

nitric acid = 27·25), into which was also placed 0·9445 gram of copper nitrate (prepared by dissolving a few of the discarded copper spheres in dilute nitric acid, evaporating the solution over the water-bath, and twice recrystallising from water). At a temperature of 25° C. no gas was evolved for 2′ after insertion of the copper sphere; it was then taken out, the liquid agitated mechanically, and the sphere again introduced; no gas was evolved for 2′ 50″. A further quantity of 2 grams urea, dissolved in 2 c.c. water, was introduced and the experiment repeated; no gas was evolved for 7′ 30″. These experiments show that even in the presence of a small quantity of copper nitrate the addition of comparatively small proportions of urea will temporarily prevent the solution of the copper. It also appeared that when once the reaction had set in it could not be stopped, even though there was present in the acid more than sufficient urea to destroy the nitrous acid which might have been formed. For after agitation of the liquid the copper was again unattacked by the acid. If, then, the apparently purely local formation of nitrous acid could be prevented, it might be possible to prevent also any reaction between the copper and nitric acid from taking place.

To test this point, 1 gram of urea dissolved in 1 c.c. of water was added to a litre of nitric acid taken from the same sample as that used in the preceding experiments. The little glass dish was more quickly rotated by means of the machine, and a very rapid current of carbonic acid passed in, so that the whole liquid was kept in violent agitation. The copper sphere was introduced, the acid being heated to 25° C. During an interval of *one hour* no gas was evolved, no blue colour imparted to the acid, no alteration of the metallic surface apparent, and the loss in weight experienced was only 0·0025 gram. Whereas, if all other conditions had remained the same, except that the nitrous acid had been initially present and had been allowed to accumulate, the loss of weight from a sphere exposing a surface of 256·85 mm. would have been about 0·3 gram (calculated from the results given in Table I, the conditions of the experiments in the two cases being practically identical). This experiment might doubtless have been allowed to proceed for a longer time with the same result.

It shows, I believe, that *pure copper will not dissolve in pure dilute nitric acid*, for though the proportion of nitric acid to that of urea present was, under these conditions, 272·5 : 1, yet the small quantity of the latter was sufficient to completely stop the reaction. It also appears that the dissolution of copper in nitric acid is dependent solely upon the local production of nitrous acid, effected probably by local electric circuits due to traces of impurities in the copper; the proportion of nitrous acid initially produced is possibly also increased by a local rise of temperature. As regards this point Dr. Russell writes (*loc. cit.*): "The nitrous acid attacks the silver,

forming silver nitrite, nitric oxide, and water, the nitric oxide thus formed reducing hydric nitrate, and forming more nitrous acid. Thus the action gets quicker and quicker. The point still remaining to be explained is, then, how the first trace of nitrous acid is formed." My experiments, *mutatis mutandis*, are quite in accord with these observations. To test the matter further, the temperature of the same sample of acid was raised to 30° C., other experiments having shown that the amount of copper dissolved off per unit area is doubled for every 4° C. No gas was evolved for 4′ after the introduction of the copper sphere, it was then taken out, and again introduced, and again no gas was evolved for 4′. The loss in weight after the 8′ insertion was only 0·0025 gram, whereas it was calculated that the amount dissolved off during the interval of time, supposing the urea to have been absent, would have been 0·11 gram.

Into the same liquid a further quantity of 2 grams urea dissolved in 2 c.c. of water was added; the acid was heated to 28—28·2° C., no gas was evolved for 18′; then as the supply of carbonic acid accidentally failed, the evolution of gas from the copper started; the loss of weight observed was 0·0015 gram, a quantity which might well have been dissolved off during the interval of time which elapsed between the commencement of the reaction and the removal of the sphere from the acid. During the experiment it was interesting to observe from time to time the formation of a bubble of gas on the metallic surface, which was brushed off by the rotation of the glass dish or the stream of carbonic acid, and consequently any further reaction stopped.

As a further confirmation another sample of acid was made up, containing a slightly greater proportion of free acid, viz., 28·2 per cent., sp. gr. $\frac{18}{18}$ = 1·1723; a larger sphere was taken presenting an area of 322·38 sq. mm.

The acid was heated to 27° C.; and 2 grams urea dissolved in 2 c.c. water were added. No gas was evolved for 50′, and the loss of weight observed was only 0·0015 gram.

Several other similar experiments have been made with similar results, which, however, it is not considered necessary to quote.

The conclusions which I venture to think may fairly be drawn from the observations herein described are:—

(1.) The view of Professor Armstrong is corroborated that the primary product of the reduction of nitric acid by copper is nitrous acid; the latter is formed immediately after any chemical change has taken place.

(ii.) If this initial formation of nitrous acid be prevented by suitable means *copper by itself will not dissolve in dilute nitric acid by itself.*

It would follow that the production of nitric and nitrous oxides with nitrogen, in proportions varying according to the conditions, is due to subsequent changes occurring between nitrous acid and cupric or cuprous nitrate or nitrite in presence of nitric acid.

I propose to continue these researches, substituting other metals for copper, especially those which are supposed to yield primarily nitrous acid.

In conclusion, I would express my thanks to Mr. Vernon Harcourt for the suggestion which proved of so much value, and to the authorities of the University for affording me facilities for this investigation.

[*Postscript.*—Since the above was written experiments have been made in which the reaction between the copper and dilute nitric acid (sp. gr. = 1·1723), heated to 27° C., was successfully prevented for some time, even in absence of urea, by substituting a current of air for that of carbonic acid.—*May* 22*nd*, 1889.]

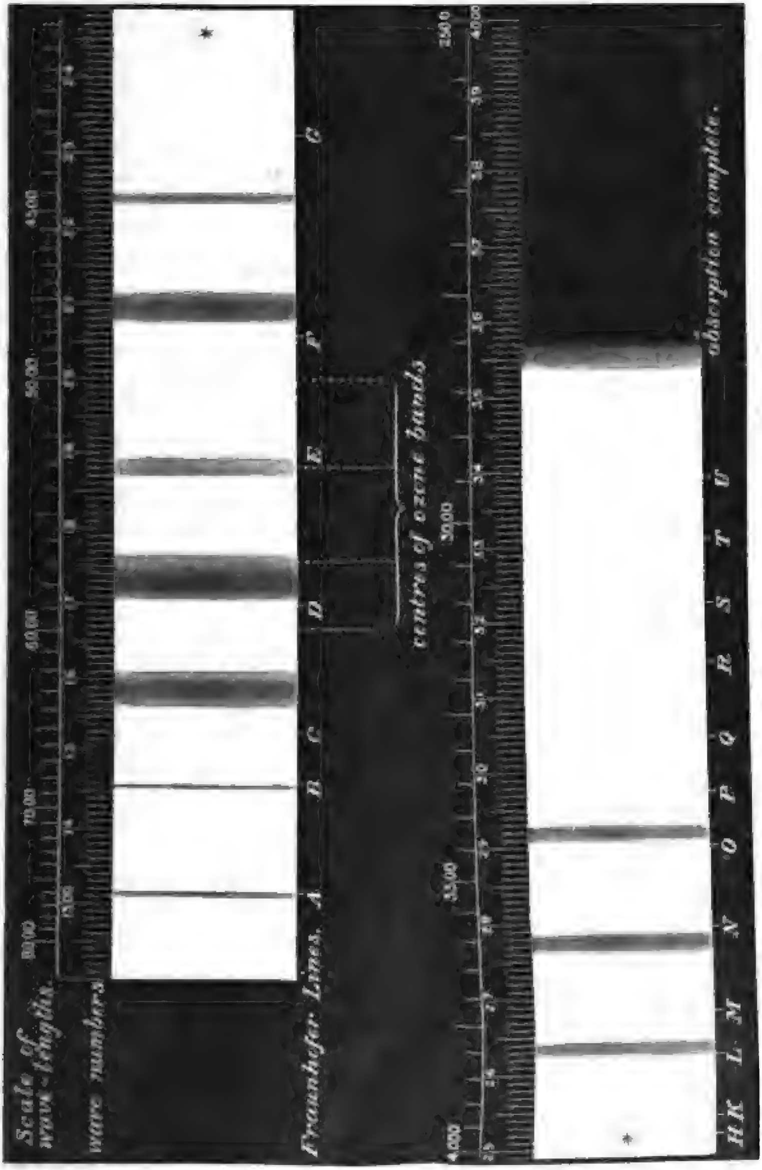
VI. "Notes on the Absorption-Spectra of Oxygen and some of its Compounds." By G. D. Liveing, M.A., F.R.S., Professor of Chemistry, and J. Dewar, M.A., F.R.S., Jacksonian Professor, University of Cambridge. Received May 23, 1889.

The absorption-spectrum of oxygen has engaged attention not only on account of the important part which that element plays in the world, but because of the remarkable character of that absorption, so strongly marked, exhibiting bands of two essentially different classes, and extremely variable under varying circumstances of condensation and combination.

It may be expected that the study of it will reveal something new as to the nature of the molecular changes brought about by different circumstances, physical and chemical.

We have already published notes of some of our experiments on this subject ('Phil. Mag.,' September, 1888), and these confirm generally the observations of Egoroff, Janssen, and Olszewski.

The accompanying diagram represents the absorption of 18 metres of ordinary oxygen gas at a pressure of about 97 atmospheres, that is, of a mass of oxygen rather greater than is contained in a vertical column of equal section of the earth's atmosphere. Under the circumstances of the experiment the absorptions A and B are very black, and the lines of which they are composed appear much broader than in the ordinary solar spectrum. The other bands are all diffuse at their edges, and, so far as we have observed, unresolvable into lines. It will be noted that the complete absorption of the


Scale of wave-lengths.
wave numbers
Fraunhofer Lines.
A B C D E F G
centres of ozone bands
H K L M N O P Q R S T U
absorption complete.

ultra-violet rays does not extend quite so far down as the limit of the solar spectrum, though it approaches that limit. There is a diffuse edge of gradually diminishing absorption succeeding the complete absorption, and this fact, together with the rapid diminution of the extent of the complete absorption with decrease of pressure, lead us to class this absorption of the extreme rays with the diffuse bands, which, according to Janssen, increase in intensity as the square of the density of the gas. If that be so, it is unlikely that the limit of the solar spectrum is due to the absorption of ordinary oxygen. For though we may suppose interplanetary space to be pervaded by materials similar to our atmosphere, yet they must be in such a state of tenuity that, although they may to some extent reinforce A and B, they will not add sensibly to the strength of the diffuse bands. Moreover, these bands, though identical in position, so far as the blue and less refrangible part of the spectrum is concerned, with bands observed by Brewster and others in the solar spectrum, are seen much more strongly through our tubes of compressed oxygen than they appear in the solar spectrum with a low sun. The ultra-violet bands, of which the one near N appears in our photographs nearly as strong as the band just above F, and that in the indigo, have not, so far as we are aware, been noticed in the solar spectrum. Probably they would appear if photographs were taken with small dispersion when the sun was low.

As the pressure in the tube diminished, the bands rapidly faded; that in the indigo, with an oscillation-frequency or wave-number about 2240, was the first to disappear, then those near L and O and that near E. At the same time the limit of the transmitted ultra-violet light advanced from an oscillation-frequency of about 3575 at 97 atmospheres, to 3710 at 50 atmospheres and 3848 at 23 atmospheres. At 20 atmospheres the three bands above C, D, and F, respectively, were still visible, though faint. B remained visible until the pressure was reduced to 2 atmospheres, and A could still be seen, but with difficulty, when the pressure of the 18 metres of oxygen was reduced to 1 atmosphere.

When atmospheric air was substituted for oxygen we found that 7 atmospheres was the limit of pressure at which we could certainly distinguish A, and 18 atmospheres the limit at which we could see B. It is a difficult matter to say exactly when an absorption becomes invisible, but the observations on air were made under the same circumstances as those on oxygen, and the two sets of observations were fairly comparable. With air at 75 atmospheres the three bands above C, D, and F, respectively, could all be seen, but that near C only with difficulty. The mass of oxygen and its *partial* pressure in the tube was in this case less by about one quarter than that which was required to bring out the bands when oxygen alone was used. The

cause of this may be that the development of the diffuse bands depends in some degree on the total pressure of the air, and not only on the partial pressure of the oxygen in it.

The mass of oxygen which when unmixed with nitrogen made A visible would correspond to that in the tube filled with air at 5 atmospheres, and that which made B visible would correspond to air at 10 atmospheres. The differences between these pressures and those which are actually needed to render A and B visible seem too great to be ascribed to errors of observation, and seem to indicate that the addition of the nitrogen has some effect on the absorptive action of the oxygen. On the other hand, Egoroff found that he could still distinguish A when the thickness of air at ordinary pressure was reduced to 80 metres ('Compt. Rend.,' vol. 101, p. 1144). This amount of air corresponds to rather less oxygen than our tube would hold at a pressure of 1 atmosphere. Differences in the sources of light, in the spectroscope, and the observers, would, however, count for a good deal in observations of this kind.

In order to try the influence of temperature on the absorption, the shorter of our experimental tubes, 165 cm. long, was surrounded by a jacket filled with a mixture of solid carbonic anhydride and ether, which was rapidly evaporated by means of a large air-pump. By this means the temperature would be reduced to −100°. The absorption of oxygen at several different pressures up to 104 atmospheres was observed through the cooled tube. We were not, however, able to detect any increase of intensity, or other change, in the absorptions which could be ascribed to the cooling. To try the effect of an increase of temperature, the 18-metre tube was surrounded by a jacket and heated up to 100° by steam. Heating appeared to render the diffuse bands rather more diffuse and less distinct. On the whole the influence of a change of temperature of 100° either way is slight.

We have observed repeatedly the absorption of liquid oxygen in thicknesses of 8 and 12 mm. Our observations confirm those of Olszewski. 8 mm. of liquid oxygen gives plainly the three diffuse bands above C, D, and F, respectively. With a thickness of 12 mm. we were not able to detect any more.

We observed the absorption produced by liquid oxygen on the one hand when it was cooled by its own evaporation until the tension of its vapour was only equal to that of the atmosphere, that is, to a temperature of −181°, according to Olszewski, and also when the temperature of the liquid was allowed to rise under pressure up to nearly the critical temperature. There appeared to be no appreciable difference in the absorption under these different circumstances when the oxygen was completely liquid, when it was near its critical temperature, and when it was completely gaseous; so far at least as

concerns the three principal bands, which were all that could be seen in the light transmitted by the liquid in a thickness of 12 mm.

It will be observed that taking the density of oxygen at $-181\cdot4^{\circ}$ to be 1·124, as given by Olszewski, 12 mm. of the liquid would be equivalent to 9·37 metres of the gas at atmospheric pressure—hardly more than half the thickness required to make A visible. The experiments, therefore, point to the conclusion that gaseous and liquid oxygen have the same absorption-spectrum. This is a very noteworthy conclusion. For, considering that no compound of oxygen, so far as is known, gives the absorptions of oxygen, the persistency of the absorptions of oxygen through the stages of condensation to the state of complete liquidity implies a persistency of molecular constitution which we should hardly have expected.

In order to compare the absorption of ozone with that of oxygen we employed a tube 12 feet long, made of tinplate fitted with glass ends and coated with paraffin on the inside. We could not use the 18 m. steel tube on account of the action of the ozone on the metal which rapidly reduced the proportion of ozone, and also because we could not conveniently cool it. Ozonised oxygen was passed into the tin tube for some time, while the ozoniser and the tube itself were cooled with ice and salt. The lime-light, viewed through the tube, looked very blue, and also the spot of light thrown from the tube on to a sheet of white paper was equally blue, indicating a considerable absorption of the less refrangible part of the spectrum. The absorption, so far as the visible rays are concerned, appeared to be of a general character, for the spectroscope revealed only four extremely faint absorption-bands. The centres of these bands were at about the wave-numbers 1662, 1752, 1880, and 1990, and their positions with reference to the bands of oxygen are indicated in the diagram. They were so faint as to be seen only with difficulty. When the hot carbon of an arc lamp was substituted for the lime-light they were rather more distinct, but the positions of the edges were undefinable. The light of a gas-lamp was insufficient to show them, and they were no better seen with a single-prism spectroscope of low dispersive power than with the spectroscope we employed for observing the oxygen. Only one of these bands, it will be seen from the diagram, is nearly coincident with an oxygen-band, namely, that near E, the faintest of the oxygen-bands. That at wave-number 1752 overlaps the strongest oxygen-band, but not at its strongest part, and has none of the peculiar character of its shading, abruptly increasing on the less refrangible side and slowly decreasing on the other side. Photographs of the spectrum (taken through a tube with quartz ends) showed that the ozone absorbed all the rays above the wave-number 3086—a point between Q and R—while partial absorption extended below Q. We may say, therefore, that we can

trace no identity between the absorptions of ozone and those of ordinary oxygen. There is no mere displacement of the bands, such as we sometimes get when a coloured substance is dissolved in different menstrua, nor any such a resemblance as we have between the absorption-bands of the different cobaltous salts derived from different acids.

The four bands which we see to be produced by ozonised oxygen correspond fairly with the 2nd, 3rd, 5th, and 6th of the bands described by Chappuis as due to ozone ('Annales de l'École Normale,' 2nd ser., vol. 11, May, 1882). These four bands, he says, are the first to be seen. We have failed to perceive any others with the 3·66 m. tube, though the oxygen was highly ozonised and maintained at a low temperature. None of the bands were of sufficient intensity to make themselves visible on our photographic plates.

It will be noted that the absorption by ozone extends far below the limit of the solar spectrum. We found, however, that by diminishing the proportion of ozone to oxygen in the tube the limit of the transmitted light was continually advanced, as already described by Hartley. The limit of the solar spectrum may, therefore, very well be determined by the average amount of ozone in the atmosphere, as Hartley supposes. The known variations in the limit of the solar spectrum may be taken as confirmatory of this hypothesis, although the comparatively small amount of those variations is certainly less than we should have expected if they measure the changes in the proportion of ozone in the atmosphere.

The absorptions of the class to which A and B belong must be those which are most easily assumed by the diatomic molecules (O^2) of ordinary oxygen. Whether oxygen in more complex molecules, as in ozone (O^3), may be capable of taking up the corresponding vibrations cannot easily be determined because we cannot isolate ozone; but since none of the compounds of oxygen with nitrogen, hydrogen or carbon, or, so far as known, with any other element, exhibit these absorptions, it is very probable that they are peculiar to the molecule O^2. From this point of view it will be interesting to determine—as we hope to do shortly—whether liquefied oxygen, which we suppose to have more complex molecules, produces these absorptions. The corresponding spectrum of emission has not as yet been observed, probably because the agency employed to render the gas luminous breaks up the molecules into single atoms of oxygen.

As for the other class of absorption, the diffuse bands, since they appear to have intensities proportional to the square of the density of the gas, they must depend on a change produced by compression. This may either be the formation of more complex molecules, as for example O^4, corresponding to the deviation from Boyle's law exhibited by oxygen gas, or it may be the constraint to which the

molecules are subject during their encounters with one another. Increase of temperature would affect the former, tending to diminish the number of complex molecules formed at a given pressure, but would have no effect on the latter, for though the number of encounters of the molecules in a given interval of time would be greater the higher the temperature, yet so long as the volume was unaltered the ratio of the duration of an encounter to that of free motion would be sensibly unaltered. So far as any change due to temperature has been observed, it is that a rise of temperature slightly weakens the diffuse absorptions.

Reverting to the compounds of oxygen, none of them show the absorptions of oxygen, not even the general absorption of the ultra-violet rays. Some of them, such as water and carbon dioxide, appear quite transparent to ultra-violet rays, while in others, such as nitrous oxide, which show a general absorption of the ultra-violet rays, the limit of transparency is different from that of oxygen. In other respects we may say that there is no resemblance between the absorptions of the compounds of oxygen and those due to oxygen itself. Some of the former have very definite and characteristic absorptions, such as the well-known spectra of the peroxides of nitrogen and chlorine, and we must regard these as indicating the rates of vibration which the molecules NO^2 and ClO^2 respectively are capable of easily taking up. The absence of the absorptions due to oxygen from all compounds of oxygen seems to indicate either that chemical combination is not, as has been supposed by some chemists, a temporary relation in which the molecular groupings are continually breaking up, to be formed anew with ever-changing elementary atoms; or, that the periods of dissociation are very small compared with the periods of association. For otherwise we should expect that such compounds of oxygen as CO^2 and NO^2 must always have amongst their molecules some molecules identical with those of oxygen and capable of taking up vibrations of the same period. At least we must conclude that little, if any, of the oxygen of these and other compounds is ever out of the influence of the other components.

We have re-examined the absorption-spectrum of N^2O^4 at various temperatures, and agree to the conclusion of Bell ('Amer. Chem. Journ.,' vol. 7, p. 32) that N^2O^4, whether liquid or gaseous, effects only a general absorption at either end of the spectrum, and that the selective absorptions observed with it are due to the presence of NO^2.

In order to obtain pure N^2O^4, the tube in which the liquid was sealed was placed in a freezing mixture, and a large part of the liquid frozen; the remaining liquid was then drained as completely as possible into the other end of the tube, and sealed off.

It should be observed that the crystals of N^2O^4 appear colourless,

and that when they are melted the liquid and superincumbent vapour are of a very pale yellow colour. As the temperature rises both liquid and vapour become, as is well known, of a deep orange, and finally of a dark, reddish-brown colour. We examined the spectra produced by two thicknesses of liquid and vapour—(1) by that contained in a narrow tube about 1 mm. in diameter, and (2) by that in a tube about 1 cm. in diameter. At 15° to 20° the vapour in the narrow as well as in the wider tube showed the well-known absorption-spectrum of fine, dark lines; no absorption by the liquid in the narrow tube could be detected, and the liquid in the wide tube showed no fine lines, but several faint, very diffuse bands, unresolvable into lines with a spectroscope of three prisms. These bands had their maxima in places where the fine lines of the vapour were most intense and most closely set, so that it might be inferred that they were due to similar molecules in both cases, but that in the liquid the vibrations of these molecules were no longer sharply defined but modified by the constraint arising from the liquid state. Some parts, however, of the spectrum of the vapour, where the lines were closely set, did not appear to be represented by any definite bands in the liquid. The liquid absorbed a good deal of blue light in a continuous manner, while the vapour only absorbed it selectively. At the red end the limit of the visible spectrum was lower for the liquid than for the gas, that is, there was more absorption of red light by the vapour than by the liquid, so much so that below a certain point the absorption by the vapour appeared continuous.

The narrow tube was next immersed in a wider tube full of glycerine, which was gradually heated. As the temperature rose, the colour of both liquid and vapour deepened, the absorptions of the vapour were stronger, and the liquid gave the same bands as had been before observed with the greater thickness. At still higher temperature the absorption of blue light, both by liquid and vapour, diminished sensibly, until at 85° the groups of lines in the blue had pretty well disappeared from the spectrum of the vapour. In fact, at 85° there was no sensible difference between the actions of liquid and vapour on blue light, it seemed only some continuous absorption. At the red end the difference between the liquid and vapour remained quite as strongly marked as at lower temperatures, if anything, more so; and the absorptions in the orange, yellow, and green were unaltered. At 90° the lines of the vapour in the green began to fade, and at 100° they were very faint; but those in the orange, as well as the corresponding diffuse bands in the liquid, were as strong as before. There was still considerably more absorption of red light by the vapour than by the liquid, as if there were a strong absorption-band in the red of the vapour which was absent in the liquid.

As the temperature rose to 110° all the lines in the vapour had become faint, and at 115° they were no longer discernible, and there

was no difference between the spectra of liquid and vapour except in the red, and even here the difference was less marked than at lower temperatures. At 130° no distinction was observable between the spectra of liquid and vapour, there were no lines or bands in either, but a good deal of general absorption. Liquid and vapour were dark, and appeared much of a colour, but the meniscus at the junction was quite evident. The tube was further heated to 155°, but no further change was noticed in the spectrum. On gradually cooling the tube, at 112° the least refrangible band in the orange was seen coming in both in vapour and liquid, diffuse in both. At 100° the usual lines were well seen in the orange, yellow, and citron of the vapour, faint lines in the green, and none in the blue; and subsequently the appearances presented on heating followed in the reverse order.

A solution of N^2O^4 in carbon bisulphide gave, in a thickness of 7 or 8 cm., diffuse absorption-bands in the green and citron, ill-defined as in liquid N^2O^4 and in corresponding positions. In a thickness of 1 cm. these bands were no longer visible.

These observations bear out the supposition that pure N^2O^4 is without selective absorption of the visible rays, and that the absorption observed is due to NO^2, both in the vapour and liquid, this absorption being modified in the liquid by the state of solution in which the molecules have much less freedom. As the temperature rises the proportion of NO^2 increases, and at the same time the density of the vapour increases and the freedom of motion of the molecules is diminished, they are less able to assume the more rapid vibrations, and those which they do assume become less sharply defined, so that the lines fade into bands and ultimately into a general absorption.

Taking Willard Gibbs's expression ("Equilibrium of heterogeneous substances," 'Connecticut Acad. Trans.,' vol. 3, p. 239) for the density D, in terms of the pressure in atmospheres p, at temperature $t°$

$$D = 3{\cdot}178 + \theta - \sqrt{\{\theta(3{\cdot}178 + \theta)\}},$$

where

$$\log_{10}\theta = 9{\cdot}47056 - \frac{3118{\cdot}6}{t+273} - \log_{10}p,$$

as deduced from Deville and Troost's experiments, we find the density of NO^2 at 140° and 50 atmospheres, equal to 2, *i.e.*, equal to the density of N^2O^4 vapour at 60° and 1 atmosphere.

Dewar and Ansdell found the critical temperature for N^2O^4 to be 156°.

VII. "Note on the Photographic Spectra of Uranus and Saturn." By WILLIAM HUGGINS, D.C.L., LL.D., F.R.S., and Mrs. HUGGINS. Received June 5, 1889.

Uranus.—In 1871 I had the honour to communicate to the Royal Society an account of the examination of the visible spectrum of Uranus.* The visible spectrum of this planet is remarkable, as it is seen to be crossed by several strong lines of absorption. Six of these dark bands are shown in a diagram which accompanies the paper, and their approximate positions in the spectrum are given. The spaces between the dark bands appear bright by contrast, and might suggest at first sight bright bands. I was unable to use a slit sufficiently narrow to enable me to determine whether the bright parts of the spectrum contain the Fraunhofer lines, which would be the case if Uranus, like the other planets, shines by reflected solar light.

The spectrum of this planet was carefully examined in 1872 by Vogel,† whose results are in accordance with my earlier ones. He observed some fainter lines or bands, in addition to those given in my paper. Vogel was unable to obtain evidence of the Fraunhofer lines. His observations agree with mine in placing a dark band at the position of F in the solar spectrum.‡

In consequence of the Fraunhofer lines not having been seen, a presumption has arisen that Uranus may shine, in part at least, by emitted light.

It appeared to me that this question might be answered by photography. With an exposure of two hours, I obtained on June 3 a photograph of the spectrum of the planet from a little above F to beyond N in the ultra-violet. A pair of sky spectra, one on each side of the planet's spectrum, were taken on the same plate.

The spectrum of Uranus, though fainter, shows all the chief Fraunhofer lines seen in the comparison spectra, and is clearly solar. I have not been able to detect any indications of bright lines, nor of any strong bands or groups of absorption, such as those in its spectrum from F to C.

There can be no doubt that the spectrum of Uranus, at least, from a little above F to beyond N in the ultra-violet, is due to reflected solar light. I have not yet been able to re-examine the visible spectrum of the planet.

Saturn.—In 1864, I gave an account of an examination of the

* 'Roy. Soc. Proc.,' vol. 19, p. 488.

† 'Untersuchungen über die Spectra der Planeten,' Leipzig, 1874.

‡ Measures of some of the bands were made at Greenwich in 1882. See 'Greenwich Spectroscopic and Photographic Results,' 1882, p. 33.

visible spectrum of this planet and its rings. In my paper on the "Photographic Spectra of Stars,"* I described the photographic spectra of Venus, Jupiter, and Mars. About a year later I took a photograph of the spectrum of Saturn and his rings, but as it did not present any new features, but was purely solar, I have not given any description of it.

The favourable position of Saturn this year for obtaining a photograph in which the spectra of the ansæ of the rings could be seen distinct from the spectrum of the ball and of the part of the ring crossing it, determined me to take some photographs of the planet and its rings.

I have adopted the plan described in 1880, in which the planet is photographed while the sky is sufficiently bright to give a faint daylight spectrum on the plate. Any additional lines or other modifications of solar light due to the planet's atmosphere can in this way be easily detected.

In the photographs taken this year the slit was so placed upon Saturn that the spectrum consists of three distinct parts, the middle part being formed by the light from the ball, and the part of the ring across it, and on both sides of this spectrum the spectra of the ansæ. The planet was kept upon the same part of the slit with sufficient exactness to keep these three spectra distinct, and from encroaching upon each other, and therefore if any difference existed between them it could be detected.

The exact correspondence of the Fraunhofer lines in the spectrum of the planet and its rings with those of the sky spectrum is clearly shown, but I am unable now, as I was in 1881, to detect any lines, dark or bright, other than those which are also present in the sky spectra. The spectrum on the plate extends from a little above F to beyond N in the ultra-violet.

I am trying to obtain enlargements of the spectra of Saturn and Uranus to serve as illustrations to this note. If they can be done so as to admit of reproduction, I will do myself the honour to present them to the Royal Society.

[We have observed since, the visible spectrum of Uranus, but under unfavourable conditions, the planet being low and the sky not dark. These observations confirm me strongly in the opinion I formed in 1871 that the brighter parts of the spectrum appear so as an effect of contrast, and do not represent emitted light. In the moments of best vision the spectrum on both sides of the brighter parts appeared to be darkened by groups of lines which give a heightened effect by contrast to the less obscured parts between them.

* 'Phil. Trans.,' 1880, p. 669.

At moments, we were conscious of dark lines crossing the spectrum, but the unfavourable conditions under which the observations were made prevented us from ascertaining by measurement or otherwise, whether any of these lines were Fraunhofer lines.—July 5.]

VIII. "The Physical Properties of Vulcanised India-rubber." By A. Mallock. Communicated by Lord Rayleigh, Sec. R.S. Received May 9, 1889.

Considering the wide use now made of india-rubber, it seems curious that the elastic constants which define its properties should not be as well known as the corresponding quantities for iron or brass.

The only published quantitative measure, however, with which I am acquainted, relating to the subject, is contained in a paragraph of Thomson and Tait's 'Natural Philosophy' (p. 230, Part II, New Edition), where the resilience of vulcanised india-rubber, *i.e.*, the amount of work restored by the substance when allowed to return to its equilibrium form, after having been stretched to a maximum short of rupture, is stated to be equivalent to its own weight raised through 1200 metres.

In 1885 I made some measures of the value of Young's modulus for india-rubber, and also examined the effect of continued strain on the material, but at that time I was not aware how much different kinds of india-rubber differed from one another in these respects, and the experiments were made on one kind of vulcanised rubber only, namely, a soft grey sort, which when cut, shows small spots of a yellowish-grey scattered throughout its substance. This year I resumed the experiments, using specimens of three different kinds of vulcanised india-rubber made at Silvertown. The specimens were cut from a sheet half an inch thick, and were square in section, and one foot long.

One was a soft grey kind, apparently identical in properties with that experimented on in 1885. The next was the well-known red sort, and the third a dark grey, much harder and stiffer than the two former.

On these specimens experiments were made to determine the three elastic constants, viz., Young's modulus, the simple rigidity, and volume elasticity. The apparent viscosity was also measured, and the behaviour of the materials under great strains, and strains continued for long periods, observed.

Young's modulus and the simple rigidity were each measured in two ways, statically and dynamically. The statical measurements being made by observing the extension and angle of torsion produced by a known force and moment; while for the dynamical measures the

frequencies were noted of the vibrations which the respective elasticities produced when acting on a known mass and moment of inertia.

As might be expected, the values obtained in these two ways do not agree, those given by the dynamical method being in all cases greater than the statical values.

The volume of elasticity can be deduced from the values of Young's modulus and the simple rigidity by the equation

$$\kappa = \frac{qn}{3q-9n},$$

where κ is the volume elasticity, q Young's modulus, and n the simple rigidity.

But since κ for india-rubber is very large, q is very nearly equal to $3n$, and the measures of q and n must be very accurate to make this formula of any use. The volume elasticity, therefore, was determined by direct measurement.

The mean values deduced from all the experiments are given in the table at the end of the paper. The values in this table refer to small strains.

When the extensions and distortions are large the values of the constants alter enormously, and the results are exhibited better by diagrams than numerically.

One property possessed by india-rubber, and to which part of the difference between the dynamic and static values of the elasticities is due, is that when strained by a given force, the extension due to the force increases gradually, rapidly at first, and then more and more slowly for many days. The difference between the extension at the first moment after the application of the force and the limit to which the extension tends is proportional to the extension, and the rate at which the extension takes place an exponential function of the time elapsed since the application of the force.

When the force is removed, the contraction takes place gradually in the same way, but not at the same rate, the constant multiplying the time in the exponent being different in the two cases. On the other hand, if the extension, not the force, is given, the force diminishes according to an exponential law as the time elapsed since the extension increases; and if the extension be quickly reduced, until the force is nothing, and then maintained constant, a contractile force will appear, and increase with the time until it reaches the amount due to difference between the length the moment after reduction and the natural length.

The material appears, in fact, to take a subpermanent set, which ultimately becomes a definite fraction of the extension to which it is subjected.

DIAGRAM I.

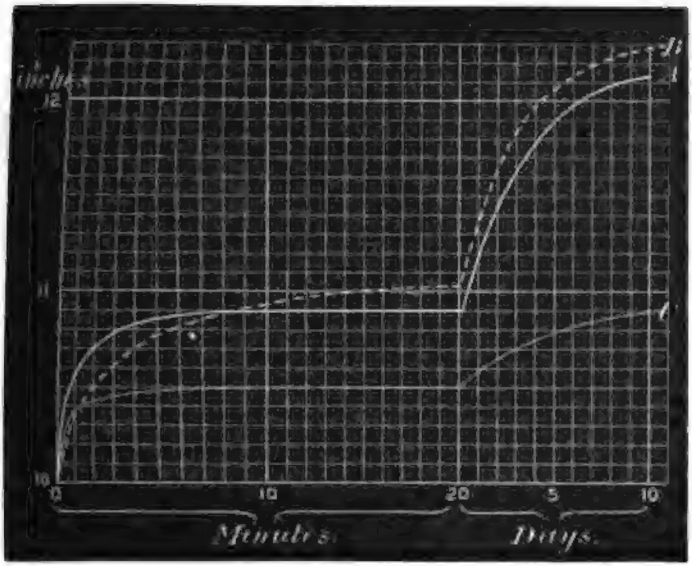
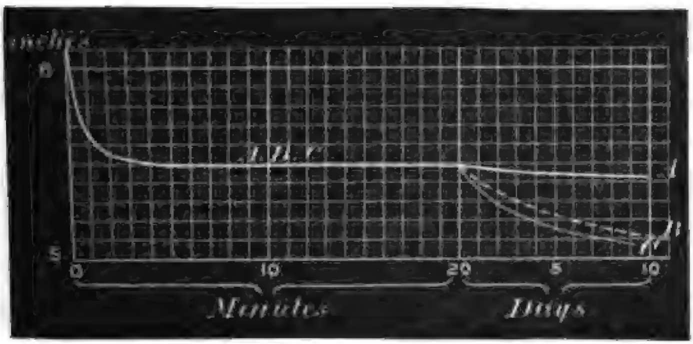


DIAGRAM II.



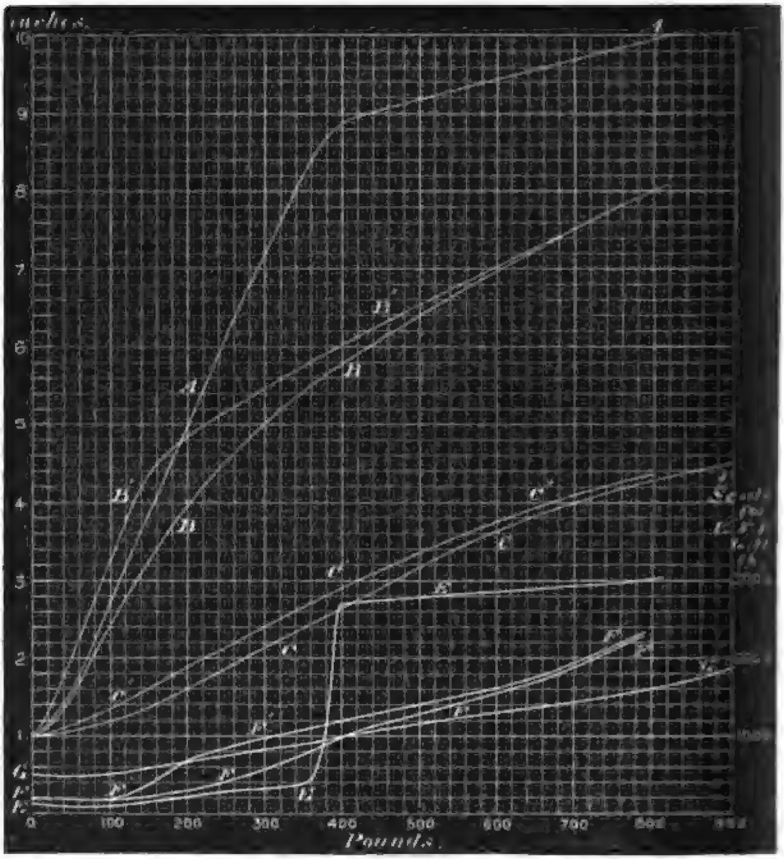
Diagrams I and II illustrate these properties, which are probably possessed to a very minute degree by many substances usually considered perfectly elastic.

From these diagrams it will be seen that the two elastic constants, viz., Young's modulus and the rigidity, are not completely defined unless the time is given for which the force calling them into play acts.

Another property of india-rubber, especially conspicuous in the soft grey kind, is that when stretched to a certain point the resistance to

further extension increases very rapidly, so rapidly indeed as to suggest that the structure of the material brings some sort of mechanical stop into action. See Diagram III.

DIAGRAM III.



Several mechanical mixtures such as putty (chalk and oil), damp clay, and sand and water exhibit similar properties. If a lump of putty be well rolled or beaten it will be found to be slightly elastic, but beyond the elastic limit to be easily stretched for a certain distance and then to become almost hard, at the same time the appearance of the surface changes from a smooth, oily character to a dull granular one.

The explanation in this case is that the hard particles of the mixture are, in its undisturbed state, separated each from its neighbours by a wall of fluid of finite thickness. When the material is distorted

the particles separate from one another in one direction and approach one another in a direction at right angles to this. As long as this approach merely involves the flow of the intervening fluid, the distortion takes place with comparative ease; but when the approach of the particles brings them into actual contact with one another, the conditions change. There is no longer a store of fluid between, say, the vertical layers of particles which can be drawn on to supply the increased distance between the horizontal layers, and if the strain is augmented it must imply either a distortion of the hard particles themselves or an increase of volume of the whole mass.

The latter is what happens in the cases just mentioned, the dull surface being the result of the fluid being sucked or rather pushed inwards by atmospheric pressure to supply the extra volume required in the interior, thus leaving the surface comparatively dry.

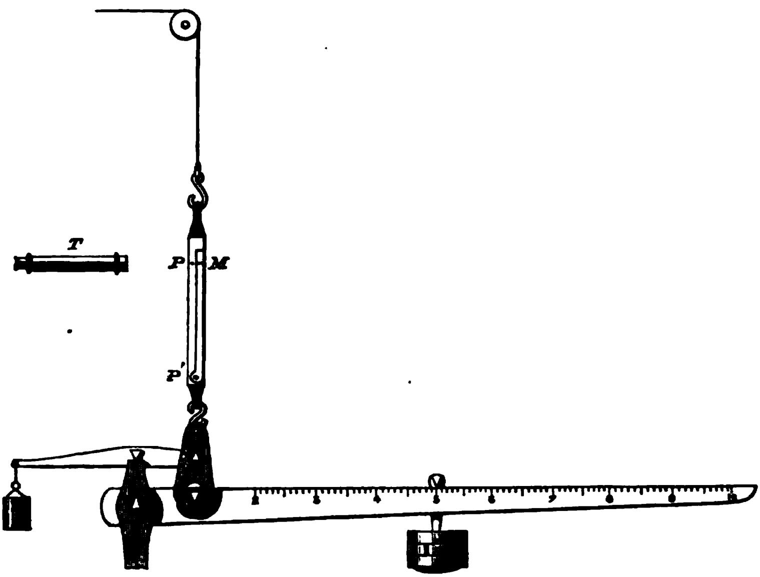
The dry patch which is seen for a short time to surround fresh footsteps on some kinds of wet sand, is an example of the same kind of action.

I will now describe the various experiments by which the results given in the table were obtained.

(1.) *Statical Measure of Young's Modulus.*

The apparatus used is shown in fig. 1. The specimen of india-rubber is attached at one end to the balance beam and at the other to

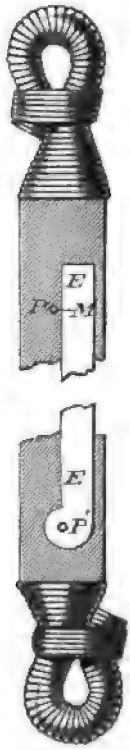
FIG. 1.



a cord passing over a pulley, by means of which it can be subjected to any desired strain.

Two very fine pins P, P′ (fig. 2), were fixed in the india-rubber at a distance of 10 inches from one another, and a thin strip of ebonite, E,

FIG. 2.



having near one end a hole the size of the pin, and a mark, M, 10 inches from the hole, was then placed against the india-rubber with the pin P′ passing through the hole; thus, when unstrained, the pin P was exactly on the same level as M; when the india-rubber was strained the extension PM was measured by the cathetometer T—

Let w = stretching force,

l = unstrained distance between P and P′,

l' = distance between P and P′ under the action of w,

s = sectional area when length is l,

s' = " " " l',

q = Young's modulus.

Then $w = q\dfrac{l'-l}{l}s$,

and since india-rubber is nearly incompressible, $ls = l's'$,

hence $$q = \frac{l'w}{s(l'-l)}.$$

To show the sort of agreement among themselves of the measures made in this way, I subjoin a table showing the results of five experiments, chosen at random from many others, on each of the kinds of india-rubber used, the units being inches and pounds.

Soft grey. $l = 10.$ $s = 0{\cdot}2690.$		Red. $l = 10.$ $s = 0{\cdot}2307.$		Hard grey. $l = 10.$ $s = 0{\cdot}2625.$	
$l'-l.$	$q.$	$l'-l.$	$q.$	$l'-l.$	$q.$
0·228	124·9	0·163	161·1	0·088	497·0
0·385	125·3	0·345	163·0	0·078	491·2
0·530	129·2	0·471	166·9	0·115	502·3
1·77	123·9	1·430	166·5	0·156	495·7
4·85	114·0	3·390	164·5	0·360	463·5

The lowest values for q are those given by experiments in which the stretching force acted for the longest time.

There is evidence also, which appears more strongly in the results represented by Diagram III, that q diminishes with the extension until the stretched length is about 3/2 times the natural length.

(2.) *Young's Modulus. Dynamical Measure.*

AP (fig. 3) is a pendulum. The strip of india-rubber DC was held rigidly at D, and attached at C to the arm AB bracketed out from the pendulum.

The experiments were made by observing the period of the pendulum with the india-rubber attached, and noting the difference between this and the natural period of the pendulum.

The india-rubber was, of course, initially strained a little, and the amplitude of the vibrations used was never great enough to make the strain vanish.

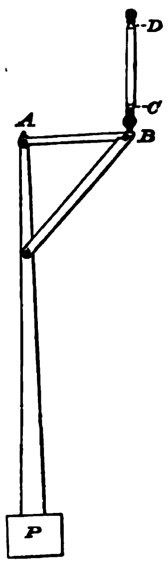
Let T_0 be the natural period of the pendulum,
λ = length of equivalent simple pendulum,
T_1 = period of pendulum with india-rubber attached,

$AB = r$,
l = natural length of india-rubber,
l_1 = DC = length of do. when attached to pendulum,
s = natural sectional area of do.,
W = weight of pendulum.

Then q as before being Young's modulus,

$$q = \frac{Wl_1^2\lambda(T_0^2 - T_1^2)}{r^2 T_1^2 l_0 s}.$$

FIG. 3.



The following are examples of the measures thus made:—

λ = 65·2 in. r = 13·5 in. l_0 = 11·2 in. W = 10·2 lb. T_0 = 2·581 sec.

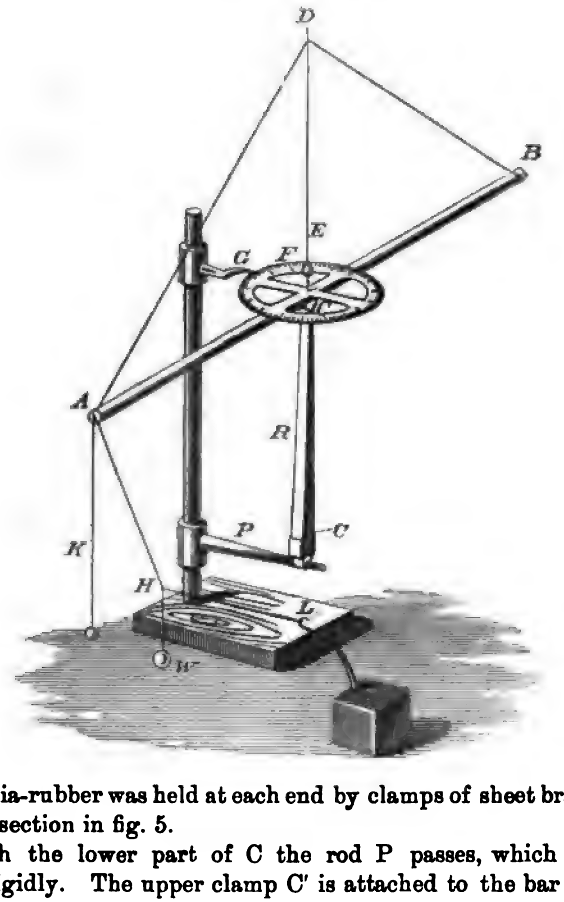
Soft grey. s = 0·2690.			Red. s = 0·2307.			Hard grey. s = 0·2625.		
T_1.	l_1.	q.	T_1.	l_1.	q.	T_1.	l_1.	q.
1·725 1·75 1·72	11·35	193·1	1·788 1·784 1·791	12·25	217·75	1·315 1·33 1·30	11·3	500·7

It will be noticed that the values of q thus obtained are much greater for the soft grey and red varieties than those obtained statically, and the chief part of this difference is due to their not having time to take the subpermanent set which they would acquire if the period was very long, but in part also it must be due to a thermodynamic cause.

(3.) *Simple Rigidity. Statical Measure.*

The arrangement shown in fig. 4 was used for both the statical and dynamical measures of the rigidity.

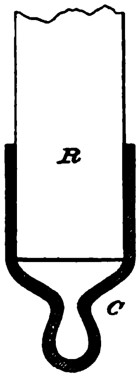
Fig. 4.



The india-rubber was held at each end by clamps of sheet brass C, C′ shown in section in fig. 5.

Through the lower part of C the rod P passes, which fixes its position rigidly. The upper clamp C′ is attached to the bar AB, this bar being suspended in a horizontal plane by two silk threads from the point D. A small plumb-bob is also hung from D to facilitate the centering of the upper clamp and the divided circle F. When the

Fig. 5.



adjustments are complete, the axis of the india-rubber is the continuation of the line DE.

In the statical measures two silk threads were attached to A, one carrying the plumb-bob K, and the other the small weight W. W was drawn to one side as shown, care being taken that the horizontal projection of AH was at right angles to AB, and that HL was horizontal. The distance HK was then measured with a scale, and the angle through which the moment due to the horizontal component of force acting along AH turned the bar AB was read on the divided circle F.

The section of the specimens of india-rubber used in these experiments being approximately square, and the reaction against torsion of a square prism being 0·883 that of a circular cylinder of the same area,* it follows that, since the torsional rigidity of similar prisms varies as the fourth power of the linear dimensions of their section, therefore the circular cylinder which has the same torsional rigidity as a square prism whose side is a, has a radius equal to $(a/\pi)\sqrt[4]{0{\cdot}883}$.

In fig. 4 let $AB = 2R$,

$CC' = l =$ length of the india-rubber,

$HK = x$,

$AK = \lambda$,

$\phi =$ angle through which the india-rubber is turned, expressed in circular measure,

$s =$ sectional area of india-rubber,

$W =$ weight hung from H.

Then if $n =$ coefficient of rigidity

and $r = (s^{\frac{1}{2}}/\pi)(0{\cdot}883)^{\frac{1}{4}}$,

$$n = \frac{2RWlx}{\pi\phi r^4\lambda}.$$

* See Thomson and Tait, 'Nat. Phil.,' vol. 1, Part II, p. 257. New Edition.

The measures of n from each specimen are given below. Many experiments were made, all agreeing very closely.

$2R = 28{\cdot}4$ in. $l = 11{\cdot}2$ in. $\lambda = 10{\cdot}75$. $W = 100$ grains.

Soft grey. $r = 0{\cdot}1560$ in.			Red. $r = 0{\cdot}1607$ in.			Hard grey. $r = 0{\cdot}1547$ in.		
x.	ϕ.	n.	x.	ϕ.	n.	x.	ϕ.	n.
2·25	0·785	65·68	2·3	0·915	50·76	4·20	0·612	161·4
2·3	0·80	65·51	3·16	1·268	50·33	5·05	0·73	162·4

(4.) *Rigidity, Dynamical Measure.*

The small weights hung from A being removed, the periods of the torsional oscillations of AB about DE were observed.

Let T = the time of oscillation,
w = weight of AB,
g = acceleration of gravity in inches and seconds.

Then for the other quantities involved, the notation being the same as in (3),

$$n = \frac{8\pi w R^2 l}{3T^2 r^4 g}.$$

For both the grey kinds of india-rubber the period varied considerably with the arc of vibration, owing partly to the extinction of the vibration being so rapid. The results were as follows:—

Soft grey.			Red.			Hard grey.		
Arc.	T.	n.	Arc.	T.	n.	Arc.	T.	n.
60°	13·2	80·6	60°	14·8		60°	9·4	156
30	12·7	to	30	14·9	56·85	30	8·8	to
10	12·0		10	14·8		10	8·55	
5	11·5	127·3	5	14·9		5	8·4	202·6

What the explanation is of the very large values for n given by this method for the soft grey india-rubber, I have not been able to find out.

Both torsional measures give n greater for the soft grey than for the red, whereas by the measures of Young's modulus, which should be very nearly equal to $3n$, n is considerably greater for the red.

It is possible that there may be a kind of "grain" in the sheets of the soft grey india-rubber, and that as the distortion produced by the extension in Experiments (1) and (2) is not in the same direction as that due to the torsion in Experiments (3), (4), the origin of the difference is to be looked for in this quarter.

(5.) *Young's Modulus for Large Extensions.*

Diagram III gives the results of these experiments. They were made with the apparatus shown in fig. 1. The actual measures were made on strips of $(\frac{1}{8})^2$ inch section, which were cut from the larger pieces in a planing machine by a sharp thin knife, wetted with dilute caustic soda. The sectional area of the strips so cut was exceedingly uniform, and its smallness was convenient, as it allowed of moderate forces being used to produce the required strains, which were increased until the breaking strain was reached.

In the diagram the results are reduced to what they would have been had the piece of india-rubber operated upon been a cube of one inch when unstrained.

The ordinates of the curves A, B, B′, C, C′, are the lengths which such cubes of soft grey, red, and hard grey india-rubber would respectively assume when stretched by forces represented by the abscissæ. In the curves B, C, the readings were taken as rapidly as possible, while in B′, C′, an interval of two minutes was allowed between each successive addition to the strain.

There were from thirty to fifty observations made for each curve. In the case of the soft grey, it did not seem to make much difference whether the readings were taken quickly or otherwise.

Let x and l be the strained and natural lengths of the india-rubber, and y the stretching force, then $q = ldy/s'dx$, and if the material is incompressible, $q = l'dy/sdx$.

By this equation the curves E, F, G were deduced from A, B, C, to show the variation of q with the extension. It is worth while to observe that since if q remain constant for all extensions,

$$F = q\frac{l'-l}{l'}A$$

$$l' = \frac{qlA}{qA-F},$$

so that with q constant, if a stretching force be applied equal per unit area to Young's modulus, the extension will be infinite.

The breaking strains for the different specimens were found to be

Soft grey........	8100	pounds per square inch, nearly.
Red	6400	
Hard grey.......	4400	

The section is that at the moment of rupture.

These numbers, therefore, are not the forces required to break a length of india-rubber of one square inch section when unstrained. To obtain the force requisite for this purpose, the numbers given above must be divided by the extensions of the unit length at the moment of rupture. They are given directly by the termination of the curves A, B, C, and are about 820 lbs. for all three kinds.

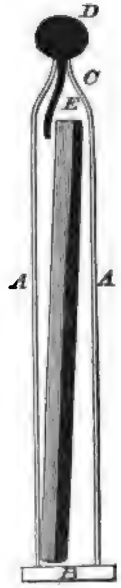
The tensile strength, however, is dependent in some measure on the time for which the force is applied, a long-continued application of force causing rupture when the force itself is not sufficient to produce the maximum extension. This is particularly noticeable in the case of the hard grey india-rubber.

(6.) *Volume Elasticity.*

There was some difficulty in obtaining a direct measure of this coefficient, owing to the very large pressures which have to be employed to produce any measurable compression.

The plan which succeeded best was to enclose the india-rubber in a

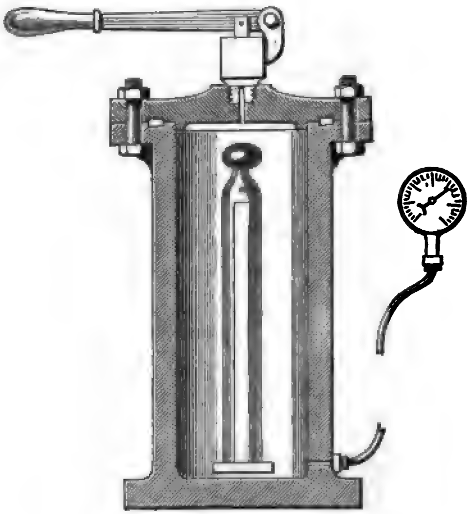
Fig. 6.



glass tube, A A, fig. 6, the lower end of which was ground flat and cemented to a small plate of thick glass, B. The other end of the tube was drawn out to a neck, the aperture being about 0·1 inch in diameter.

After the india-rubber was enclosed and the plate B cemented on, A was filled with water, great care being taken that no air bubbles were enclosed. The neck was then closed by a ball of soft wax and turpentine mixture, D, and the whole immersed in water in a cast-iron cylinder (fig. 7), when it was subjected to a pressure of about 550 lbs. per square inch.

FIG. 7.



Under this pressure the water and india-rubber are somewhat compressed. Since the wax and turpentine is soft, the glass tube experiences but little difference of internal and external pressure, the mixture flowing in through the neck of the tube and forming a long filament, E, the volume of which represents the compression of the contents of the tube.

When the pressure is gradually removed this filament is partly expelled, but retains its shape, and its length and sectional area being known, the data are supplied for computing the volume elasticity of the india-rubber.

Let V' be the volume of the tube A,
V do. india-rubber,
v do. the intruded wax,
κ' the volume elasticity of water,
κ do. do. india-rubber,
p the pressure in cylinder.

Then
$$\kappa = \frac{V\kappa' p}{\kappa' v - p(V' - V)}.$$

As a test of the accuracy of the method, the volume elasticity of water was measured. The value found for κ' was 296,000 lbs. per square inch, a result which is not far from the truth.

The values for india-rubber were

	κ.
Soft grey......	198,000
Red..........	115,000
Hard grey.....	940,000

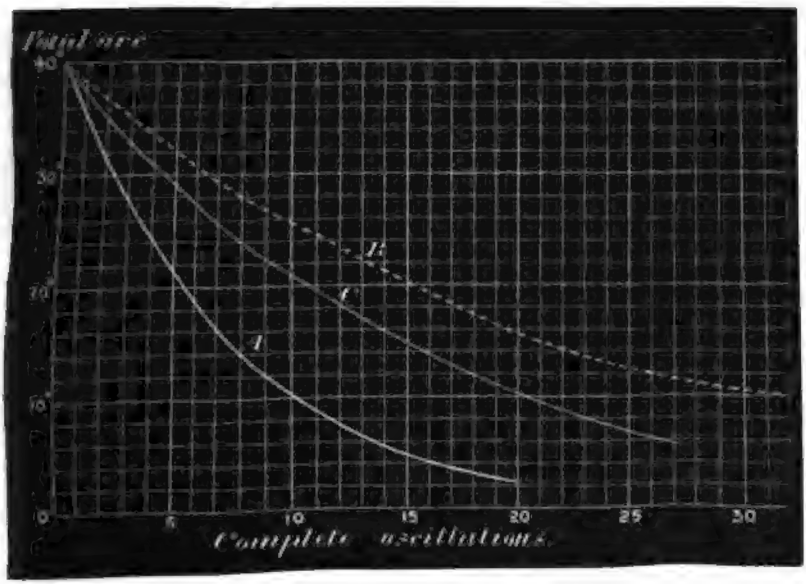
These values are the means of all the experiments after the first. The first application of pressure, however, always produced a much more considerable compression, and if the volume of elasticity had been deduced from the first experiment only, its value would have been about half that given above in the case of the soft grey and red; and for the hard grey, about one-eighth.

(7.) *Viscosity.*

The rate at which the vibration in Experiment (4) died away was used to determine the coefficient of viscosity.

Diagram IV gives the "curves of extinction" for the three kinds of india-rubber. The ordinate of the curve is at each point, the ampli-

DIAGRAM IV.



tude which the vibration would have if the phase of the vibration was such that a maximum distance from the position of rest occurred at that point.

Let λ be the logarithmic decrement of the vibration, c_1 and c_n the amplitudes of the 1st and nth vibrations respectively,

then $$\lambda = \frac{1}{n-1} \log_e 10 \log \frac{c_1}{c_n},\text{*}$$

and if p be the coefficient of viscosity,

$$p = \frac{4R^2wl\lambda}{3g\pi r^4 T}.$$

The symbols with the exception of λ having the same meaning as those in Experiment (4).

The values found for p were

	p.	
Soft grey......	13·74	pounds per square inch.
Red..........	2·578	
Hard grey	7·725	

The coefficient p represents the tangential force required to distort a cube of one inch of the material at the rate of one inch per second, independently of that necessary to overcome the elastic reaction, on the assumption that the viscous resistance to distortion varies as the rate of distortion. Part of the apparent viscosity however must be due to the difference of the rates at which the sub-permanent set is produced and removed.

(8.) The densities of the specimens were

Soft grey......	1·289
Red..........	1·407
Hard grey	2·340

(9.) *Chemical Composition.*

I had no means at my disposal here of making a good analysis, but a rough determination of the percentage of sulphur was obtained by decomposing a known weight of each kind with caustic soda and nitre, and observing the quantity of barium chloride required in each case to precipitate the sulphate formed. The results were

	Sulphur, per cent.
Soft grey........	5·7
Red............	2·1
Hard grey	3·8

* Maxwell's 'Electricity and Magnetism,' vol. 2, 239.

Both the grey india-rubbers yield a considerable ash when burnt. The hard grey, as is apparent from its density, containing a large percentage of inorganic matter.

The following table gives the mean of all the experiments.

Table showing the Physical Properties of three kinds of Vulcanised India-rubber.

Description of india-rubber.	Density.	Young's modulus.		Simple rigidity.	
		Statical.	Dynamical.	Statical.	Dynamical.
Soft grey........	1·289	124	195	65	80 to 127
Red............	1·407	166	217	50	57
Hard grey........	2·340	495	500	158	156 to 202
	Volume elasticity.	Viscosity.	Limit of stretching of unit length.	Breaking strain.	Breaking strain for a square inch of unstrained material.
Soft grey........	198,000	13·74	9·9	about 8100	about 820
Red............	115,000	2·578	7·3	6400	820
Hard grey........	940,000	7·725	4·4	4400	820

The units employed throughout this paper are the inch, pound, and second.

The Society adjourned over the Whitsuntide Recess to Thursday, June 20th.

Presents, *June* 6, 1889.

Transactions.

Baltimore:—Johns Hopkins University. Studies in Historical and Political Science. Ser. 7. Nos. 5–6. 8vo. *Baltimore* 1889. The University.

Batavia:—Bataviaasch Genootschap van Kunsten en Wetenschappen. Notulen. Deel XXVI. Aflev. 3. 8vo. *Batavia* 1888; Tijdschrift voor Indische Taal-, Land- en Volkenkunde. Deel XXXII. Aflev. 5. 8vo. *Batavia* 1889; Nederlandsch-Indisch Plakaatboek, 1602–1811. Deel V. 8vo. *Batavia* 1888. The Society.

Transactions (*continued*).

Brisbane:—Royal Geographical Society of Australasia (Queensland Branch). Proceedings and Transactions. Vol. III. Part 2. 8vo. *Brisbane* 1889. The Society.

Helsingfors:—Sällskapet för Finlands Geografi. Fennia. Vol. I. 8vo. *Helsingfors* 1889. The Society.

Jena:—Medizinisch-Naturwissenschaftliche Gesellschaft. Jenaische Zeitschrift für Naturwissenschaft. Bd. XXIII. Heft 2–3. 8vo. *Jena* 1889. The Society.

Liverpool:—Astronomical Society. Journal. Vol. VII. No. 6. 8vo. *Liverpool* 1889. The Society.

London:—Geological Society. Quarterly Journal. Vol. XLV. No. 178. 8vo. *London* 1889. The Society.

London Mathematical Society. Proceedings. Vol. XX. Nos. 346–348. 8vo. *London* 1888. The Society.

Madrid:—Real Academia de Ciencias Exactas Físicas y Naturales. Anuario. 1889. 12mo. *Madrid.* The Academy.

New York:—Linnean Society. Abstract of the Proceedings for 1888–89. 8vo. [*New York.*] The Society.

Palermo:—Circolo Matematico. Rendiconti. 1889. Fasc. 2. 8vo. *Palermo.* The Circolo.

Paris:—Comité International Permanent pour l'Exécution Photographique de la Carte du Ciel. Bulletin. Fasc. 3. 4to. *Paris* 1889. The Académie des Sciences.

Société d'Encouragement pour l'Industrie Nationale. Annuaire. 1889. 8vo. *Paris.* The Society.

Société Mathématique de France. Bulletin. Tome XVII. No. 1. 8vo. *Paris* 1889. The Society.

Siena:—R. Accademia dei Fisiocritici. Atti. Ser. 4. Vol. I. Fasc. 1–2. 8vo. *Siena* 1889. The Academy.

Stockholm:—Kongl. Vetenskaps-Akademie. Öfversigt. Årg. 46. No. 3. 8vo. *Stockholm* 1889. The Academy.

Tokio:—College of Science, Imperial University. Journal. Vol. II. Part 5. 4to. *Tōkyō* 1889. The College.

Trieste:—Società Adriatica di Scienze Naturali. Bollettino. Vol. XI. 8vo. *Trieste* 1889. The Society.

Turin:—R. Accademia delle Scienze. Atti. Vol. XXIV. Disp. 8–10. 8vo. *Torino* 1888–89. The Academy.

Utrecht:—Physiologisch Laboratorium der Utrechtsche Hoogeschool. Onderzoekingen. Derde Reeks. Vol. XI. 8vo. *Utrecht* 1889. The School.

Observations and Reports.

Columbus:—Ohio Meteorological Bureau. Report for March, 1889. 8vo. *Columbus.* The Bureau.

Observations, &c. (*continued*).

Dorpat:—Sternwarte. Meteorologische Beobachtungen. 1888. November—December. 8vo. [*Dorpat*]; Bericht über die Ergebnisse der Beobachtungen an den Regenstationen der Kaiserlichen Livländischen Gemeinnützigen und Ökonomischen Societät für das Jahr 1887. 4to. *Dorpat* 1889.
The Observatory.

Germany:—Königl. Preussisches Geodätisches Institut. Polhöhenbestimmungen aus dem Jahre 1886. 4to. *Berlin* 1889; Gewichtsbestimmungen für Seitenverhältnisse in schematischen Dreiecksnetzen. 4to. *Berlin* 1889. The Institute.

Glasgow:—Mitchell Library. Report. 1888. 8vo. *Glasgow* 1889.
The Library.

London:—Admiralty Office. Hydrographic Department. Reports of the Results of an Examination by the Officers of H.M.S. "Rambler" of the Slopes and Zoological Condition of Tizard and Macclesfield Banks. 1888. Folio. *London* 1889; Report on the Bore of the Tsien-Tang Kiang. 8vo. *London* 1888.
The Department.

Middlesex Hospital. Reports of the Medical, Surgical, and Pathological Registrars for 1887. 8vo. *London* 1888.
The Hospital.

Stationery Office. Report on the Scientific Results of the Voyage of H.M.S. "Challenger." Vol. XXXI. Zoology. 4to. *London* 1889. The Office.

University. Calendar for 1889–90. 8vo. *London* 1889.
The University.

Lyme Regis:—Rousdon Observatory, Devon. Meteorological Observations. 1886, 1888. 4to. *London* 1887, 1889.
Mr. Cuthbert E. Peek.

Milan:—Reale Osservatorio di Brera. Pubblicazioni. Num. 34. 4to. *Milano* 1889. The Observatory.

Portugal:—Commissão dos Trabalhos Geologicos. Flora Fossil de Portugal. Monographia do Genero Dicranophyllum (Systema Carbonico). 4to. *Lisboa* 1888. The Commission.

Turin:—Osservatorio della Regia Università. Bollettino. Anno 1887. Obl. 8vo. *Torino* 1889. The Observatory.

Washington:—Bureau of Navigation. Pilot Chart of the North Atlantic Ocean, May, 1889. [Sheet.] *Washington.*
The Bureau.

U.S. Signal Office. Bibliography of Meteorology. Part I. Temperature. [Lithogr.] 4to. *Washington* 1889.
The Office.

Buckman (S. S.) and J. F. Walker. On the Spinose Rhynchonellæ (Genus *Acanthothyris*, d'Orbigny), found in England. [Excerpt.] 8vo. *York* 1889. The Authors.

Calendar of Wills proved and enrolled in the Court of Husting, London, A.D. 1258—A.D. 1688. Edited, with Introduction, by Reginald R. Sharpe, D.C.L. Part I. 8vo. *London* 1889. The Corporation of the City of London.

Delaurier (E.) Nouvelle Théorie de l'Univers. Folio. *Paris* [1889] Théories Nouvelles des Causes des Maladies et des Fermentations Folio. *Paris* [1889]. The Author.

Downing (A. M. W.) The Eclipses of Jupiter's Satellites. 8vo. *London* 1889. The Author.

Fonblanque (E. Barrington de). Annals of the House of Percy. Two Vols. 8vo. *London* 1887. The Duke of Northumberland, K.G.

Galloway (W.) Sinking Appliances at Llanbradach. 8vo. *London* [1889]. The Author.

Harley (G.), F.R.S. What is a Geyser? 8vo. *Edinburgh* [1889]. The Author.

Inglis-Parsons (J.) The Arrest of Growth in four cases of Cancer by a powerful Interrupted Voltaic Current; a case of Pelvic Tumour treated by Galvano-Puncture: Cured. 8vo. *London* 1889. [Excerpt.] The Author.

Jones (T. R.), F.R.S. Notes on the Palæozoic Bivalved Entomostraca. No. 27. 8vo. [*London*] 1889. The Author.

Jones (T. R.), F.R.S., and C. D. Sherborn. A Supplemental Monograph of the Tertiary Entomostraca of England. 4to. *London* 1889. The Authors.

Loomis (E.) Contributions to Meteorology (Chapter 3). Revised edition. 4to. *New Haven* 1889. The Author.

Major (F.) Spacial and Atomic Energy. Part I. 8vo. *London* 1889. The Author.

O'Driscoll (F.) Notes on the Treatment of Gold Ores. 8vo. *London* 1889. The Author.

Paganelli (A.) Risposta alle Osservazioni ed Appunti della Civiltà Cattolica sulla Cronologia Rivendicata. 8vo. *Prato* 1889. The Author.

Peschka (G. A. V.) Freie Perspektive. Bd. 1–2. 8vo. *Leipzig* 1888–89. The Author.

Prince (C. Leeson) The Summary of a Meteorological Journal, 1888. Folio. [*Crowborough*] 1889. The Author.

Rambaut (A. A.) A New Determination of the Latitude of Dunsink Observatory. 4to. *Dublin* 1889. With two other Excerpts in 8vo. The Author.

Schwœrer (E.) Le Milieu Interstellaire et la Physique Moderne. 8vo. *Paris* 1889. The Author.

Steenstrup (J.), For. Mem. R.S. Mammuthjæger-Stationen ved Předmost. 8vo. *Kjöbenhavn* 1889. With two other Excerpts in 8vo. The Author.

Tejera (M.) Origen y Constitución Mecánica del Mundo. 8vo. *Barcelona* 1889. The Author.

Volante (A.) Eureka Areostatica ai pie' della Ferrea Corona. 4to. *Torino* 1888. The Author.

Walker (J. F.) Communications [extracted] from the Yorkshire Philosophical Society's Report, 1888. 8vo. The Author.

NOTICES TO FELLOWS OF THE ROYAL SOCIETY.

A Monograph of the Horny Sponges, by Dr. R. von Lendenfeld, is now published. The price to the public is £3; to Fellows of the Royal Society, £2. As the edition is limited to 500 copies, Fellows should order their copies at once of the Assistant Secretary.

The following are now offered *to Fellows only* at reduced prices:—

"Catalogue of the Scientific Books in the Library of the Royal Society"—

Part 1.	Transactions, Journals, &c......	4*s*. net.
Part 2.	General Catalogue............	7*s*. „
The two Parts	....	10*s*. „

"International Polar Expedition. Fort Rae Observations." 10*s*. 6*d*. net.

"Catalogue of Scientific Papers," in sets:—

	Net prices. £	*s*.	*d*.	
Vols. I—VI in Half-Morocco at . . .	3	10	0	the set.
*Vol. I, Half-Morocco, and II—VI, Cloth.	2	14	6	„
Vols. VII, VIII in Half-Morocco at . .	1	10	0	the two.
„ „ „ in Cloth	1	1	0	„

The Library will be open after the month of September, from 11 A.M. to 6 P.M. (Saturdays 1 P.M.)

Philosophical Transactions.—The Fellows are now permitted to give standing orders for the delivery of the volumes of 'Philosophical Transactions' to their addresses as published. The order must be in writing, and should be sent to the Assistant Secretary.

On application to Messrs. Harrison and Sons, 46, St. Martin's Lane, Fellows and others can have the separate numbers of the 'Philosophical Transactions' bound in cloth case for 2*s*. 6*d*., or can obtain cloth cases for binding, price 1*s*. 6*d*.

List of Fellows.—Fellows who desire to have the Annual List posted to them every year as soon as issued should send their names to the Assistant Secretary.

A printed post-card of the papers to be read at each meeting will be sent weekly to any Fellow upon application to Messrs. Harrison and Sons, 46, St. Martin's Lane, W.C.

* Vol. I in cloth is sold out.

HARRISON AND SONS, 45 & 46, ST. MARTIN'S LANE, W.C.,

AND ALL BOOKSELLERS.

PROCEEDINGS OF

THE ROYAL SOCIETY.

VOL. XLVI. NO. 283.

CONTENTS.

June 20, 1889.

For continuation of Contents see 2nd page of Wrapper.

Price Four Shillings and Sixpence.

CONTENTS (*continued*).

ERRATA, VOL. 46.

Page 211, line 14 from bottom, *for* "1887" *read* "1877."
" 212 " 21 " "phenylhydrazide" " "phenylhydrazone."

Steenstrup (J.), For. Mem. R.S. Mammuthjæger-Stationen ved Předmost. 8vo. *Kjöbenhavn* 1889. With two other Excerpts in 8vo. The Author.

Tejera (M.) Origen y Constitución Mecánica del Mundo. 8vo. *Barcelona* 1889. The Author.

Volante (A.) Eureka Areostatica ai pie' della Ferrea Corona. 4to. *Torino* 1888. The Author.

Walker (J. F.) Communications [extracted] from the Yorkshire Philosophical Society's Report, 1888. 8vo. The Author.

June 20, 1889.

Professor Sir G. GABRIEL STOKES, Bart., President, in the Chair.

Mr. John Aitken, Dr. E. Ballard, Mr. A. B. Basset, Mr. Horace T. Brown, Mr. Latimer Clark, Mr. Lazarus Fletcher, Mr. W. B. Hemsley, Dr. C. T. Hudson, Mr. E. B. Poulton, Professor W. J. Sollas, Mr. Herbert Tomlinson, and Professor G. F. Yeo were admitted into the Society.

The Presents received were laid on the table, and thanks ordered for them.

The President announced to the Meeting that it had that afternoon been resolved by himself and the Council to address a letter to the Lord Mayor of London, expressing sympathy with his attempt to obtain some public recognition in this country of services rendered by M. Pasteur to science and humanity, and that the officers, with Sir James Paget, Sir Joseph Lister, Sir Henry Roscoe, and Professor Lankester, had been appointed to represent the Society at the meeting which the Lord Mayor had called for July 1st.

The following Papers were read:—

I. "On the Cavendish Experiment." By C. V. Boys, A.R.S.M., F.R.S., Assistant Professor of Physics at the Normal School of Science, South Kensington. Received May 29, 1889.

The Cavendish experiment for determining the constant of gravitation, from which the density of the earth may be calculated, is so well known that there is no occasion to describe it. This experiment,

devised by the Rev. John Mitchell, F.R.S., was first carried out by Cavendish,* and has been since performed by Reich,† Baily,‡ and Cornu and Baille,§ who have all followed very closely the arrangement of Cavendish.

Owing to the very small value of the constant of gravitation, all these experimentalists have aimed at increasing the sensibility as much as possible. With this object, a long beam carrying at its ends considerable masses has been suspended by a very long and very fine wire. The attracting masses have been made as large as possible, and they have been brought almost into contact with the sides of the long box in which the beam is suspended. Cornu, it is true, has reduced the dimensions of all the parts to about one-quarter of the original amount. His beam, an aluminium tube, is only half a metre long, and it carries at its ends masses of $\frac{1}{4}$ lb. each, instead of about 2 lb. as used by Cavendish. This reduction of the dimensions to about one-quarter of those used previously is considered by Cornu to be one of the advantages of his apparatus, because, as he says, if the period of oscillation is unchanged, then the sensibility is independent of the mass of the suspended balls, and is inversely as the linear dimensions. I do not quite follow this, because, as I shall show, if all the dimensions are increased or diminished together the sensibility will be unchanged. If only the length of the beam is altered and the positions of the large attracting masses, so that they remain opposite to and the same distance from the ends of the beam, then the sensibility is inversely as the length.

The other improvements introduced by Cornu are the use of mercury for the attracting masses which can be drawn from one pair of vessels to the other without coming near the apparatus, the use of a metal case connected with the earth to prevent electrical disturbances, and the electrical registration of the movements of the index on the scale which was placed 560 cm. from the mirror. The period of oscillation which has been used has varied between 398 seconds (Cornu) and 840 seconds (Cavendish). Cavendish found that with the very inconvenient period of 1800 seconds the balls knocked against the side of the case.

The difficulty that has been met with has been the perpetual shifting of the position of rest, due partly to the imperfect elasticity or fatigue of the torsion wires, and partly, as Cavendish proved experimentally, to the enormous effects of air currents set up by temperature differences in the box, which with large apparatus it is impossible to prevent. In every case the power of observing was in

* 'Phil. Trans.,' 1798, p. 469.

† 'Comptes Rendus,' 1837, p. 697.

‡ 'Phil. Mag.,' vol. 21, 1842, p. 111.

§ 'Comptes Rendus,' vol. 76, p. 954; vol. 86, pp. 571, 699, 1001.

excess of the constancy of the effect actually produced. The observations of Cornu are the only ones which are comparable in accuracy with other physical measurements, and these, as far as the few figures given enable one to judge, show a very remarkable agreement between values obtained for the same thing from time to time.

Soon after I had made and found the value of quartz fibres for producing a very small and constant torsion, I thought that it might be possible to apply them to the Cavendish apparatus with advantage. Professor Tyndall, in a letter to a neighbour written some months ago, expressed the conviction that it would be possible to make a much smaller apparatus in which the torsion should be produced by a quartz fibre. Last summer I began to prepare an instrument with a working beam five millimetres long, but other experimental work obliged me to put this on one side for a time. I have lately examined the theory of this instrument in some detail, and as I find that in many particulars there is an advantage in departing from the arrangement that has always been employed, I have lately prepared two pieces of apparatus, which on trial fully bear out the results of this inquiry.

I shall, therefore, first give a short account of the principles that should be followed in the design of the Cavendish apparatus, and then describe the results which I have obtained up to the present time.

As I have already stated, the sensibility of the apparatus is, if the period of oscillation is always the same, independent of the linear dimensions of the apparatus. Thus, if there are two instruments in which all the dimensions of one are n times the corresponding dimensions of the other, then the moment of inertia of the beam and its appendages will be as $n^5 : 1$, and, therefore, the torsion also must be as $n^5 : 1$. The attracting masses, both fixed and movable, will be as $n^3 : 1$, and their distance apart as $n : 1$. Therefore, the attraction will be as n^6/n^2 or $n^4 : 1$, and this is acting on an arm n times as long in the large instrument as in the small, therefore the moment will be as $n^5 : 1$, that is, in the same proportion as the torsion, and so the angle of deflection is unchanged.

If, however, the length of the beam only is changed, and the attracting masses are moved until they are opposite to and a fixed distance from the ends of the beam, then the moment of inertia will be altered in the ratio $n^2 : 1$, while the corresponding moment will only change in the ratio of $n : 1$, and thus there is an advantage in reducing the length of the beam until one of two things happens, either it is difficult to find a sufficiently fine torsion thread that will safely carry the beam and produce the required period, and this, I believe, has up to the present time prevented the use of a beam less than half a metre in length, or else when the length becomes nearly equal to the diameter of the attracting balls, they then act with such an

increasing effect on the opposite suspended balls, so as to tend to deflect the beam in the opposite direction, that the balance of effect begins to fall short of that which would be due to the reduced dimensions if the opposite ball did not interfere. Fig. 1 will make the meaning more clear. ab is the beam of the ordinary apparatus with a ball at

FIG. 1.

each end. M is one of the attracting masses, and the other one occupies a symmetrical position on the opposite side of the centre O, but as the relations of each with the moving system are identical, it will be sufficient to consider only one.

As the beam is supposed to become shorter, the small balls will occupy successively the places $a'b'$ and $a''b''$, while the large mass M will take the corresponding places shown by the dotted circles at M′ and M″. When it has reached the position M″, at which the line joining its centre with O makes an angle of 45° with ab, the sensibility of the combination is still increasing, but not quite so fast as it would do if the attraction on the ball b did not partly counteract the attraction on the ball a. Should this position be chosen for the mass M, then the beam of the length $a''b''$ is not the best that can be used, if it is further shortened the sensibility will be still further increased, and will become a maximum when the beam has a length equal to half $a''b''$, that is, when the distance between the large balls is $2\sqrt{2}$ times the distance between the small ones. If the length of the beam is made successively equal to 1, 2, 3 . . . . . 10 tenths of the distance $a''b''$, then the corresponding deflections will be represented by the numbers in the following table:—

ab.	Deflection.
0·1	1·050
0·2	1·070
0·3	1·077
0·4	1·082
0·5	1·088
0·6	1·080
0·7	1·066
0·8	1·037
0·9	0·982
1·0	0·911

The unit deflection is that which would be produced if each large ball acted only on the small ball near it, and if the small balls occupied the positions $a''b''$.

If the position which is chosen for each attracting mass is nearer the plane of the beam than the transverse plane, that is, if the azimuth of the large masses is less than 45°, the best length of the beam will be more than half that which would bring the ends opposite the attracting masses.

It might be urged against this argument that a difficulty would arise in finding a torsion fibre that would give to a very short beam loaded with balls that it will safely carry a period as great as five or ten minutes, and until quartz fibres existed there would have been a difficulty in using a beam much less than a foot long, but it is now possible to hang a thing only half an inch long and weighing from 20 to 30 grains by a fibre not more than a foot in length, so as to have a period of five minutes. If the moment of inertia of the heaviest beam of a certain length that a fibre will safely carry is so small that the period is too rapid, then the defect can be remedied by reducing the weight, for then a finer fibre can be used, and since the torsion varies approximately as the square of the strength (not exactly because fine fibres carry heavier weights in proportion), the torsion will be reduced in a higher ratio, and so by making the suspended parts light enough, any slowness that may be required may be provided.

Practically, it is not convenient to use fibres much less than one ten-thousandth of an inch in diameter, and these have a torsion ten thousand times less than that of ordinary spun glass. A fibre one five-thousandth of an inch in diameter will carry a little over 30 grains.

Since with such small apparatus as I am now using it is easy to provide attracting masses which are very large in proportion to the length of the beam, while with large apparatus comparatively small masses must be made use of owing to the impossibility of dealing with balls of lead of great size, it is clear that much greater deflections can be produced with small than with large apparatus. For instance, to get the same effect in the same time from an instrument with a 6-foot beam that I get from one in which the beam is five-eighths of an inch long, and the attracting balls are 2 inches in diameter, it would be necessary to provide and deal with a pair of balls each 25 feet in diameter and weighing 730 tons instead of about $1\frac{3}{4}$ lb. apiece. There is the further advantage in small apparatus that if for any reason the greatest possible effect is desired, attracting balls of gold would not be entirely unattainable, while such small masses as two piles of sovereigns could be used where qualitative effects only were to be shown. Owing to its strongly magnetic qualities, platinum is unsuited for experiments of this kind.

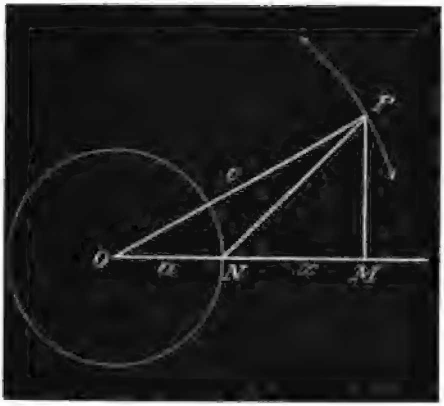
By far the greatest advantage that is met with in small apparatus is the perfect uniformity of temperature which is easily obtained; whereas, with apparatus of large size, this alone makes really accurate work next to impossible. The construction to which this inquiry has led me, and which will be described later, is especially suitable for maintaining a uniform temperature in that part of the instrument in which the beam and mirror are suspended.

With such small beams as I am now using it is much more convenient to replace the long thin box generally employed to protect the beam from disturbance by a vertical tube of circular section, in which the beam with its mirror can revolve freely. This has the further advantage that if the beam is hung centrally, the attraction of the tube produces no effect, and the troublesome and approximate calculations which have been necessary to find the effect of the box are no longer required. The attracting weights, which must be outside the tube, must be made to take alternately positions on the two sides of the beam, so as to deflect it first in one direction and then in the other. For this purpose they are most conveniently fastened to the inside of a larger metal tube which can be made to revolve on an axis coincident with the axis of the smaller tube. There are obviously two planes, one containing and one at right angles to the beam, in which the centres of the attracting balls will lie when they produce no deflection. At some intermediate position the deflection will be a maximum. Now it is a matter of some importance to choose this maximum position for the attracting masses, because, in showing the experiment to an audience, the largest effect should be obtained that the instrument is capable of producing; while in exact measures of the constant of gravitation this position has the further advantage that the only measurement which there is any difficulty in making, viz., the angle between the line joining the large masses and the line joining the small, which may be called the azimuth of the instrument, becomes of little consequence under these circumstances. In the ordinary arrangement the slightest uncertainty in this angle will produce a relatively large uncertainty in the result. I have already stated that if an angle of 45° is chosen, the distance between the centres of the large balls should be $2\sqrt{2}$ times the length of the beam, and the converse of course is true. As it would not be possible at this distance to employ attracting balls with a diameter much more than one and a half times the length of the beam, and as balls much larger than this are just as easily made and used, it will be well to find out what will be the position for maximum deflection when the centres of the attracting balls are any distance apart.

In the case already considered the problem gives rise to equations of too high an order to be readily solved, and so in the particular case referred to the result was obtained by arithmetical means. If the

effect in the nearer ball only is considered, then it is easy to find the best position for any distance of the attracting mass from the axis of motion. Let P (fig. 2) be the centre of the attracting ball, N that of

FIG. 2.



the nearer attracted ball, O the axis of motion, c and a the distances of P and N from O, and x the distance from N of the foot of the perpendicular from P on ON produced. Then the moment of N about O will be greatest when

$$x^2 + \frac{3a^2+c^2}{a}x = 2(c^2-a^2),$$

or what comes to the same thing when

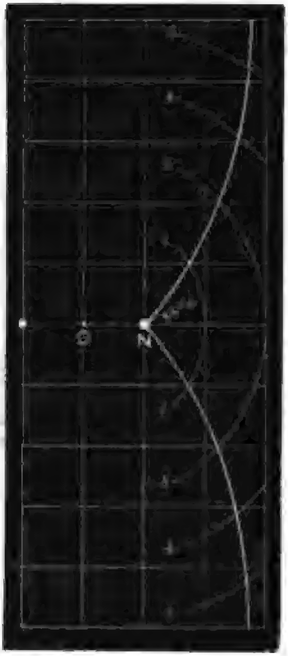
$$\cos^2\theta + \frac{c^2+a^2}{ca}\cos\theta = 3.$$

The solutions of these equations are given in the following table:—

c/a.	θ		x/a.
1	0°	0′	0
2	27	45	0·77
3	42	16	1·22
4	51	19	1·50
5	58	20	1·62
6	63	15	1·70
7	66	44	1·76
8	69	23	1·82
9	71	17	1·88
10	72	50	1·95
∞	90	0	2·00

These figures are represented by the curve in fig. 3, which shows the best position for an attracting mass at any distance from the

FIG. 3.



axis O. The inclination of this with the line ON at the point N is 35° 16′, or an angle of which the tangent is equal to $1/\sqrt{2}$. This curve also shows the best position from which a source of light at any distance from O would most brightly illuminate a small surface at N lying along ON.

If now an attracting ball is placed in a position of maximum effect with its centre on this line it will act on the further suspended ball, tending to deflect the beam in the opposite direction, and this will become more marked as the distance between the centres of the attracting balls increases, and so the increased effect which would be due to a greater attracting ball may be largely compensated by the increased action on the remote end of the beam. The azimuth at which the maximum effect is produced is also changed.

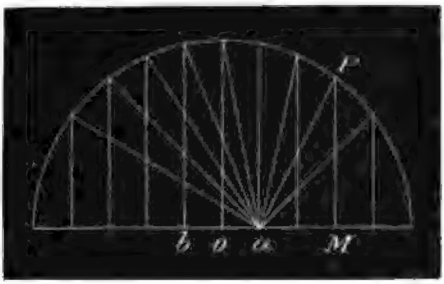
I have practically overcome this difficulty by arranging the two sides of the apparatus at different levels. Each large ball is at or near the same level as the neighbouring small ball, but one pair is removed from the level of the other by about the diameter of the large balls which in the apparatus which I have now the honour to

submit to the Society is nearly five times as great as the distance *in plan* between the two small balls.

In order to realise more fully the effect of a variety of arrangements, I have, for my own satisfaction, calculated the values of the deflecting forces in an instrument in which the distance between the centres of the attracting balls is five times the length of the beam, for every azimuth and for differences of levels of 0, 1, 2, 3, 4, and 5 times the length of the beam.

This calculation is very much facilitated by the property of the circle illustrated in fig. 4. If the diameter is divided into any number of equal parts, and perpendiculars drawn to cut the circle, then the squares of lines drawn from any one of the points on the diameter to all the intersections (including the two ends of the diameter) are in arithmetical progression, and the common difference is equal to twice

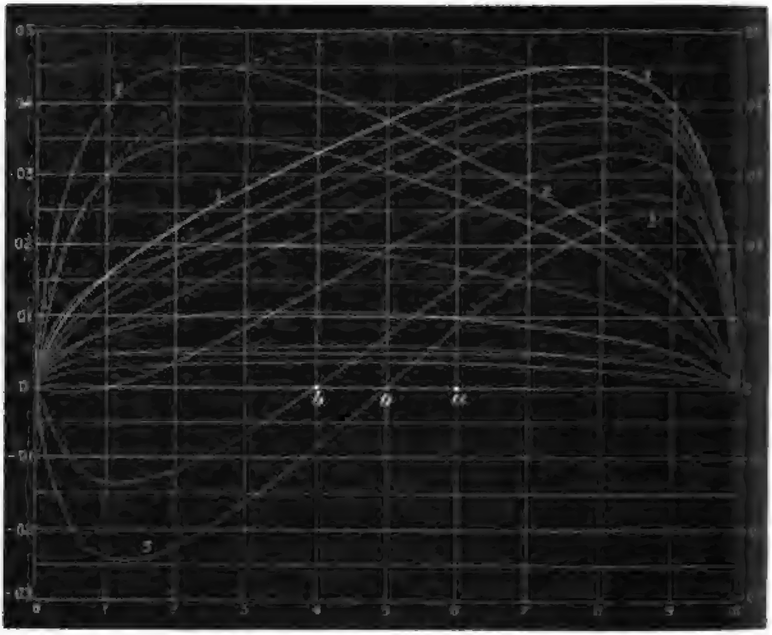
Fig. 4.



the number of parts included between that point and the centre. If the diameter is divided into ten parts, a and b are the positions of the ends of the beam, and the semicircle is the path of the centre of the large mass. When this is at any position P the resolved force at a is equal to PM/Pa^3. Now all the quantities PM^2 and Pa^2 are small whole numbers, and the squares of the true distances of P from a when a is at different levels are small whole numbers also, so that all the logarithms can be found on the first four pages of Chambers's tables. It is for this reason that it is most convenient to represent the result of the calculation on a diagram in which the abscissæ are the projections of the centre of the attracting mass on a plane passing through the centres of the small balls.

In fig. 5 the dotted circle represents the possible positions of the centre of the attracting mass, and a, b the small balls. The heavy Curve 1 shows the value of the moment due to the ball a alone. The reversed Curve 2 in the same way shows the moment of the ball b in the opposite direction when that ball is at the same level as a. The Curve 3 is the difference between these two, and from this actual resultant moments may be found. The maximum of this curve is in

FIG. 5.



a slightly different position from that of the Curve 1, and its greatest value is only just over half that of the first curve, which shows that the sensibility can be nearly doubled by lowering the ball *b* until it is too far down to be appreciably attracted by the ball which is on a level with *a*. The five curves below 2 show the effect of lowering *b* until it is 1, 2, 3, 4 and 5 times the length of *ab* below the general level, and the five curves between 1 and 3 in the same way represent the values of the balance in favour of *a* when *b* is at these different levels. The curves when drawn are instructive in that they show both the relative advantages of the various differences of levels, and from the curvature at the maximum positions the practical importance of correctly determining the azimuth. By reference to the number on the vertical scale it is also easy to directly compare the sensibility of the apparatus with any of the arrangements which have been in use. For instance, calling in every case the half length of the beam unity and the dimensions of the other parts by numbers in proportion, we have in the apparatus of Cavendish—

Equivalent distance between centres of large and small balls (8·95 inches)	0·249
Diameter of large balls (12 inches)	0·333
Comparative value of deflecting force $0{\cdot}333^3/0{\cdot}249^2$	0·596

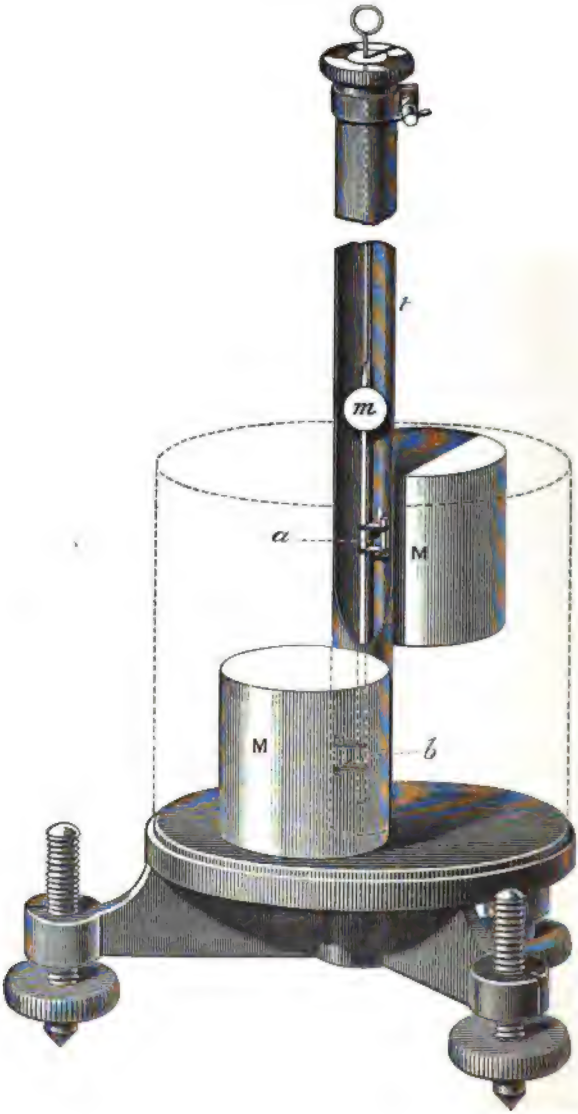
In my apparatus it is simply necessary to multiply the cube of the diameter of the large balls by the ordinate of the curve, to find on the same scale the value of the deflection. This requires that the large balls should be made of material of the same density in the two cases, and that the periods should be the same. Now the diameter of the large ball in the new apparatus is 6·4 times the length of the beam, and so the comparative value of the deflecting force is $0{\cdot}0425 \times 6{\cdot}4^3 = 11{\cdot}1$, a figure which is 18·7 times as great as the figure found for the apparatus of Cavendish. If then the large balls have the same density as those used by Cavendish and the period of oscillation is the same, the angular deflection will be 18·7 times as great.

Having now found that with apparatus no bigger than an ordinary galvanometer it should be possible to make an instrument far more sensitive than the large apparatus in use heretofore, it is necessary to show that in practice such a piece of apparatus will practically work, and that it is not liable to be disturbed by the causes which in large apparatus have been found to give so much trouble.

I have made two instruments of which I shall only describe the second, as that is better than the first both in design and in its behaviour.

The construction of this is made clear by fig. 6. To a brass base provided with levelling screws is fixed the vertical brass tube, *t*, which forms the chamber in which the small masses *a*, *b* are suspended by a quartz fibre from the pin at the upper end. These little masses are cylinders of pure lead 11·3 mm. long and 3 mm. in diameter, and the vertical distance between their centres is 50·8 mm. They are held by light brass arms to a very light taper tube of glass, so that their axes are 6·5 mm. from the axis of motion. The mirror *m*, which is 12·7 mm. in diameter, plane and of unusual accuracy, is fastened to the upper end of the glass tube by the smallest quantity of shellac varnish. Both the mirror and the plate-glass window which covers an opening in the tube were examined and afterwards fixed with the refracting edge of each horizontal, so that the slight but very evident want of parallelism between their faces should not interfere with the definition of the divisions of the scale. The large masses M, M are two cylinders of lead 50·8 mm. in diameter, and of the same length. They are fastened by screws to the inside of a brass tube, the outline of which is dotted in the figure, which rests on the turned shoulder of the base so that it may be twisted without shake through any angle. Stops (not shown in the figure) are screwed to the base, so that the actual angle turned through shall be that which produces the maximum deflection. A brass lid made in two halves covers in the outer tube and serves to maintain a very perfect uniformity of temperature in the inner tube. Neither the masses M, M, nor the lid touch the inner tube. The period of oscillation is 80 seconds.

Fig. 6.



With this apparatus placed in an ordinary room with draughts of air of different temperatures and with a lamp and scale such as are used with a galvanometer, the effect of the attraction can easily be shown to a few, or, with a lime-light, to an audience. To obtain this

result with apparatus of the ordinary construction and usual size is next to impossible, on account chiefly of the great disturbing effect of air currents set up by difference of temperature in the case. The extreme portability of the new instrument is a further advantage, as is evident when the enormous weight and size of the attracting masses in the ordinary apparatus are considered.

However, this result is only one of the objects of the inquiry which I have now the honour to submit to the Royal Society. The other object which I had in view was to find whether the small apparatus, besides being more sensitive than that hitherto employed, would also be more free from disturbances and so give more consistent results. With this object I have placed the apparatus in a long narrow vault under the private road between the Museum and the Science Schools. This is not a good place for experiments of this kind, for when a cab passes overhead the trembling is so great that loose things visibly move; however, it is the only place at my disposal that is in any degree suitable. A large drain pipe filled with gravel and cement and covered by a slab of stone forms a fairly good table. The scale is made by etching millimetre divisions on a strip of clear plate glass 80 cm. long. This is secured at the other end of the vault at a distance of 1053·8 cm. from the mirror of the instrument. A telescope 132 cm. long and with an object-glass 5·08 cm. in diameter rests on V's clamped to the wall, with its object-glass 360 cm. from the mirror. Thus any disturbance that the observer might produce if nearer is avoided and at the same time the field of view comprises 100 divisions. While the observer is sitting at the telescope he can by pulling a string move an albo-carbon light mounted on a carriage so as to illuminate any part of the scale that may happen to be in the field of the telescope. The white and steady flame forms a brilliant background on which the divisions appear in black. The accuracy of the mirror is such that the millimetre divisions are clearly defined, and the position of the cross-wire (a quartz fibre) can be read accurately to one-tenth of a division. This corresponds to a movement of the mirror of almost exactly one second of arc.

The mode of observation is as follows: When all is quiet with the large masses in one extreme position, the position of rest is observed and a mark placed on the scale. The masses are moved to one side for a time and then replaced which sets up an oscillation. The reading of every elongation and the time of every transit of the mark are observed until the amplitude is reduced to three or four centimetres. The masses are then moved to the other extreme position and the elongations and transits observed again, and this is repeated as often as necessary.

On the evening of Saturday, May 18th, six sets of readings were

taken, but during the observations there was an almost continuous tramp of art students above, producing a perceptible tremor, besides which two vehicles passed, and coals were twice shovelled in the coal cellar, which is separated from the vault in which the observations were made by only a four and a half inch brick wall. The result of all this was a nearly perpetual tremor, which produced a rapid oscillation of the scale on the cross-wire, extending over a little more than 1 mm. This increased the difficulty of taking the readings, but to what extent it introduced error I shall not be able to tell until I can make observations in a proper place.

In spite of these disturbances, the agreement between the deflections deduced from the several sets of observations and between the periods is far greater than I had hoped to obtain, even under the most favourable conditions. In order to show how well the instrument behaved, I have copied from my note-book the whole series of figures of one set, which sufficiently explain themselves.

Elongation.	Amplitude.	Decrement.	True position of rest.	Time of transit of 36·09.	Correction for transit of true position of rest.	True half period.
				h. m. s.		
15·05						
53·20	38·15	0·805	36·18	9 8 25·0	+0·08	80·2
22·48	30·72	0·808	36·20	9 45·5	−0·18	80·2
47·28	24·80	0·807	36·21	11 5·3	+0·24	80·0
27·28	20·00	0·807	36·20	12 25·8	−0·28	79·9
43·40	16·12	0·805	36·22	13 45·0	+0·41	80·1
30·42	12·98	0·806	36·21	15 6·0	−0·47	80·1
40·88	10·46	0·802	36·22	16 25·0	+0·63	79·5
32·50	8·38	0·808	36·24	17 46·0	−0·91	80·5
39·27	6·77	0·808	36·24	19 4·5	+1·13	79·8
33·80	5·47	0·814	36·26	20 27·0	−1·58	80·5
38·25	4·45		36·26	21 44·0	+1·94	
		0·8066				80·08

It will be noticed that the true position of rest is slightly rising in value, and this rise was found to continue at the rate of 0·36 cm. an hour during the whole course of the experiment, and to be the same when the large masses were in the positive or negative position. The motion was perfectly uniform, and in no way interfered with the accuracy of the experiments. It was due, I believe, to the shellac fastening of the fibre, for I find that immediately after a fibre has been attached this movement is very noticeable, but after a few days it almost entirely ceases; it is, moreover, chiefly evident when the

fibre is loaded very heavily. At the time that the experiment was made the instrument had only been set up a few hours.

The mean decrement of three positive sets was 0·8011, and of three negative sets, 0·8035. The observed mean period of three positive sets was 79·98, and of three negative sets, 80·03 seconds, from both of which 0·20 must be deducted as the time correction for damping.

The deflections obtained from the six sets of observations taken in groups of three, so as to take into account the effect of the slow change of the position of rest, were as follows:—

From sets 1, 2, and 3........	17·66 $\pm$ 0·01
,, 2, 3, and 4........	17·65 $\pm$ 0·02
,, 3, 4, and 5........	17·65 $\pm$ 0·02
,, 4, 5, and 6........	17·65 $\pm$ 0·02

An examination of these figures shows that the deflection is known with an accuracy of about one part in two thousand, while the period is known to the four thousandth part of the whole. As a matter of fact the discrepancies are not more than may be due to an uncertainty in some of the observations of half a millimetre or less, a quantity which, under the circumstances, is hardly to be avoided.

The result of these experiments is complete and satisfactory. As a lecture experiment the attraction between small masses can be easily and certainly shown, even though the resolved force causing motion is, as in the present instance, no more than the one two hundred-thousandth of a dyne (less than one ten-millionth of the weight of a grain), and this is possible with the comparatively short half period of eighty seconds. Had it been necessary to make use of such half periods as three to fifteen minutes which have been employed hitherto, then, even though a considerable deflection were produced, this could hardly be considered a lecture experiment.

The very remarkable agreement between successive deflections and periods shows that an absolute measure made with apparatus designed for the purpose, but on the lines laid down above, is likely to lead to results of far greater accuracy than any that have been obtained. For instance, in the original experiment of Cavendish there seems to have been an irregularity in the position of rest of one-tenth of the deflection obtained, while the period showed discrepancies of five to fifteen seconds in seven minutes. The experiments of Baily made in the most elaborate manner were more consistent, but Cornu was the first to obtain from the Cavendish apparatus results having a precision in any way comparable to that of other physical measurements. The three papers, published by him in the 'Comptes Rendus,' of 1878, referred to above, contain a very complete solution of some of the problems to which the investigation has given rise. The agreement between the successive values, decrement, and period is much the same that I have

obtained, nevertheless the means of the summer and of the winter observations differ by about 1 per cent.

I have not referred to the various methods of determining the constant of gravitation in which a balance, whether with the usual horizontal beam, or with a vertical beam on the metronome principle, is employed. They are essentially the same as the Cavendish method, except that there is introduced the friction of the knife-edges and the unknown disturbances due to particles of dust at these points, and to buoyancy, without, in my opinion, any compensating advantage. However, it would appear that if the experiment is to be made with a balance, the considerations which I have advanced in this paper would point to the advantage of making the apparatus small, so that attracting masses of greater proportionate size may be employed, and the disturbance due to convection reduced.

It is my intention, if I can obtain a proper place in which to make the observations, to prepare an apparatus specially suitable for absolute determinations. The scale will have to be increased, so that the dimensions may be determined to a ten-thousandth part at least. Both pairs of masses should, I think, be suspended by fibres or by wires, so that the distance of their centres from the axis may be accurately measured, and so that in the case of the little masses the moment of inertia of the beam, mirror, &c., may be found by alternately measuring the period with and without the masses attached. The unbalanced attractions between the beam, &c., and the large masses, and between the little masses and anything unsymmetrical about the support of the large masses, will probably be more accurately determined experimentally by observing the deflections when the large and the small masses are in turn removed, than by calculation.

If anything is to be gained by swinging the small masses in a good Sprengel vacuum, the difficulty will not be so great with apparatus made on the scale I have in view, *i.e.*, with a beam about 5 cm. long, as it would with large apparatus. With a view to reduce the considerable decrement, I did try to maintain such a vacuum in the first instrument, in which a beam 1·2 cm. long was suspended by a fibre so fine as to give a complete period of five minutes, but though the pump would click violently for a day perhaps, leakage always took place before long, and so no satisfactory results were obtained.

With an apparatus such as I have described, but arranged to have a complete period of six minutes, it will be possible to read the scale with an accuracy of one ten-thousandth of the deflection, and to determine the time of vibration with an accuracy about twice as great.

II. "On Time-lag in the Magnetisation of Iron." By J. A. Ewing, B.Sc., F.R.S., Professor of Engineering in University College, Dundee. Received June 18, 1889.

When any change is made to take place in the magnetic force acting on a piece of soft (annealed) iron, a considerable time elapses before the resulting change in the magnetism of the piece is complete. The sluggishness which soft iron exhibits in assuming its full magnetism when a magnetic force is imposed upon it was referred to as follows in the account which I wrote, some years ago, of experiments on the magnetic qualities of iron:—*

"Some evidence was given that, in addition to much static hysteresis, there is a small amount of viscous lagging in the changes of magnetism which follow changes of magnetising force. I repeatedly observed that when the magnetising current was applied to long wires of soft iron, either gradually or with more or less suddenness, there was a distinct creeping up of the magnetometer deflection after the current had attained a steady value, as measured by the deflection of the galvanometer through which it passed. This action was sometimes so considerable as to oblige me to wait for some minutes before taking the magnetometer reading."

In his paper "On the Behaviour of Iron and Steel under the Operation of Feeble Magnetic Forces,"† Lord Rayleigh has remarked on the same phenomenon in soft iron. In his experiments the relation of the magnetic force to the resulting magnetisation of the specimen was studied by means of a magnetometer furnished with a "compensating coil," through which the magnetising current passed, and which was so placed that its action on the needle of the magnetometer balanced the action of the iron, giving no deflection. When very feeble magnetic forces were applied to hard iron or to steel, he found that a perfect balance might be obtained by adjusting the position of the compensating coil, and so established the fact that the susceptibility to small magnetic forces, or to small changes of force, is a definite quantity, which is independent of the amount of the small change of force. He observes that with hard iron and steel the compensating coil might be set so that neither at the moment of closing the circuit of the magnetising current nor afterwards was there any deflection of the magnetometer, which means that (so far as the magnetometer can decide) the metal assumes its magnetic state instantaneously. He goes on to say that soft iron shows much more complicated effects: "When the coil was so placed as to reduce as much as possible the instantaneous effect, there ensued a drift of the

* "Exp. Researches in Magnetism," 'Phil. Trans.,' 1885, p. 569, § 52.

† 'Phil. Mag.,' March, 1887, p. 230.

magnetometer needle . . . . in such a direction as to indicate a continued increase of magnetisation. Precisely opposite effects followed the withdrawal of the magnetising force. The settling down of the iron into a new magnetic state is thus shown to be far from instantaneous. On account of the complication caused by the free swings of the needle, good observations on the drift could not be obtained with this apparatus, but it was evident that whilst most of the anomalous action was over in 3 or 4 seconds, the final magnetic state was not attained until after about 15 or 20 seconds." Lord Rayleigh then cites my observation, quoted above.

In the following experiments Lord Rayleigh's method of the compensating coil has been made use of for the purpose of examining in some detail this "drift," or "creeping," or quasi-viscous change of magnetism which follows any change in the magnetic force acting on soft iron.

The magnetometer was a light Thomson mirror directed by the horizontal component of the earth's field, and having a free period of double swing amounting to nearly 1½ seconds. The specimen of iron used in the greater number of the experiments was a straight piece of thick wire 0·404 cm. in diameter and 39·6 cm. long, over which was slipped a tube with a magnetising solenoid wound upon it. The wire was set in a vertical position, magnetically west of the magnetometer, with its top end on a level with the mirror, and generally 6 cm. distant from it. The compensating coil was wound on a wooden frame, which could be moved along a "geometric slide" towards or from the magnetometer in the east-west line through the mirror, for the purpose of balancing the magnetic effect of the iron. In some of the experiments another compensating coil was used to balance the effect on the magnetometer of the magnetising solenoid, but generally the simpler plan was followed of including the effect of the solenoid in the determination of the compensating coil's action on the magnetometer.

To prevent the vertical component of the earth's field from acting on the iron, a second magnetising solenoid was wound over the first, and a constant current of the proper strength to neutralise the earth's field was maintained in it without interruption. The main magnetising current was regulated by having in its circuit a box of resistance coils, and also the liquid slide described in my former paper.* This allowed the magnetic force to be changed either suddenly or gradually, and the slide also allowed the method of demagnetising by numerous reversals of a continuously diminishing magnetic force to be resorted to whenever it was desired to reduce the iron to a magnetically neutral state.

To soften the wire it was heated to redness by being slowly drawn

* *Loc. cit.*, § 18, p. 537.

through a Bunsen flame. After it was put in place the method of reversals was applied to extract a small amount of magnetism which it had acquired in being handled. In the experiments which I shall first describe the effects of very feeble magnetic force were examined by making and breaking the circuit of the magnetising solenoid while the current was adjusted to produce a force of less than 0·1 c.g.s. It was found that the *immediate* effect of each make and break could be balanced very exactly by adjusting the position of the compensating coil, and so long as the magnetising force was considerably less than 0·1 c.g.s. the distance at which the coil had to be set to give this balance was as nearly as possible independent of the value of the force, and was the same for "break" as for "make." The position of the coil was adjusted so that at the instant when the magnetising current was set up by pressing down a contact key, there was no sudden deflection of the magnetometer mirror to either side. When the compensation was right the spot of light simply began to drift slowly towards the side corresponding to increase of magnetism; when there was over-compensation, the spot of light gave a quiver to the opposite side before beginning to drift, and the position of the coil was adjusted by drawing it back little by little until the quiver on pressing down the key disappeared. The amount of magnetism that was balanced was afterwards measured by removing the iron, but leaving the magnetising solenoid and the compensating coil in place, and observing the deflection of the magnetometer when the same current was passed through the empty solenoid and the compensating coil. This determined the immediate magnetic effect of the magnetising current on the iron, and the subsequent creeping up of the magnetism was of course determined by observing the drifting of the magnetometer needle which had ensued after applying the current while the iron was in its place.

In the following experiment a current of 21 on the arbitrary galvanometer scale (equivalent in this case to a magnetising force of 0·044 c.g.s.) was made, after the wire had been completely demagnetised, and after the compensating coil had been adjusted to balance the immediate effect. Magnetometer readings were taken 5 seconds and 60 seconds after "make;" and at 60 seconds the current was broken, and magnetometer readings were taken 5 seconds and 60 seconds after "break." The immediate effect (balanced by the compensating coil) was equivalent to twenty-five divisions of the magnetometer scale.

Time after "make."	Magnetometer.	Time after "break."	Magnetometer.
0	0	0	13
5″	8	5″	5
60″	13	60″	0

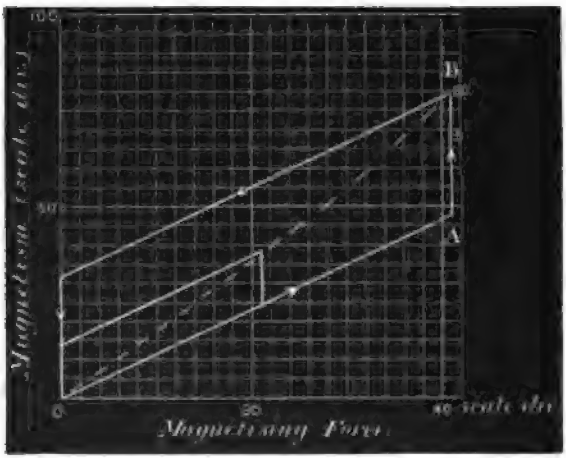
Adding to these the equivalent of the compensating coil, we see that just after the immediate magnetising force was suddenly applied, the value of the magnetism was 25, which increased after 5 seconds to 33, and after 1 minute to 38; and that when the magnetising force was suddenly withdrawn, there was at first a residual magnetism of 13, which fell to 5 in 5 seconds, and disappeared altogether in less than 1 minute.

Next a current 41 (producing a magnetising force of 0·084 c.g.s.) was made and broken in the same way. The compensating coil scarcely required to be moved from its former position, and its equivalent on the magnetometer was now 48. The column headed "total" gives the sum of the magnetometer reading and the part balanced by the compensating coil.

Time after "make."	Magnetometer.		Time after "break."	Magnetometer.
	Observed.	Total.		
0	0	48	0	31
5″	20	68	5″	13
60″	31	79	60″	4

Here out of the whole original residue of 31, a small part refused to disappear after the lapse of a minute, and it is probable that with this magnetising force some of the residual magnetism is permanent.

Fig. 1.



The above results are shown in fig. 1 where the arrows indicate the sequence of magnetic changes. One scale division of the magnetometer is here equivalent to 0·0177 c.g.s. units of $\mathfrak{I}$ (intensity of

magnetism). The magnetic force due to the solenoid may be taken as approximately equal to the whole magnetic force (although the rod was barely 100 diameters long, this length should be sufficient to approximate to endlessness when one is dealing with very low values of magnetic susceptibility). On this assumption, one scale division of the galvanometer is equivalent to 0·0021 of $\mathfrak{I}$; the initial instantaneous susceptibility, that is, the gradient $d\mathfrak{I}/d\mathfrak{H}$, is 9·9, and the initial instantaneous permeability ($d\mathfrak{B}/d\mathfrak{H}$) is 125. This value has been confirmed by a number of independent observations made with the same piece of annealed wire, and with another piece cut from the same hank and also annealed. Taking the magnetism acquired after 1 minute, the initial susceptibility as regards that is about 15.

Precisely similar results have been obtained by reversing feeble magnetic forces. So long as the forces are very small, the compensation for "reverse" is the same as for "make" and for "break," and the creeping of the magnetism in any given time after make, break, or reverse is nearly proportional to the amount of the preceding change of magnetising force.

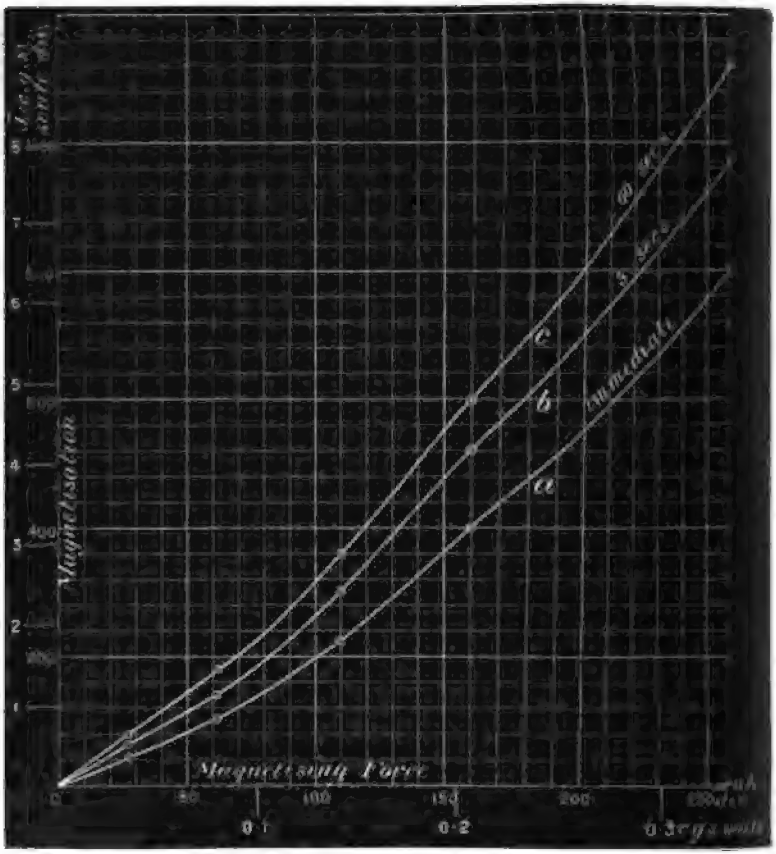
In the following experiments the magnetising force was raised to higher values, at which this proportionality no longer held good. As before, the compensating coil was adjusted for each current to balance the effect of "make," the iron being demagnetised by reversals immediately before the "make." When a stronger current was applied, the coil had to be pushed nearer the magnetometer: but up to forces of 0·3 c.g.s. or so, it was practicable to secure an instantaneous balance by doing so. Observations of the drift were taken at 5 and 10 second intervals during 1 minute.* These are given

Table I.

Time after "make."	Current.				
	27	62	110	161	261
seconds.	Magnetometer + comp. coil.				
0	47	107	224	395	798
5	65	145	304	525	974
10	72	159	327	560	1071
15	74	165	339	573	1089
20	77	169	344	581	1098
25	79	171	347	586	1104
30	79	173	350	590	1109
40	80	175	354	595	1116
50	80	177	355	598	1120
60	80	177	357	600	1124

* To make the drift large the top of the wire was this time only 4 cm. from the magnetometer.

Fig. 2.



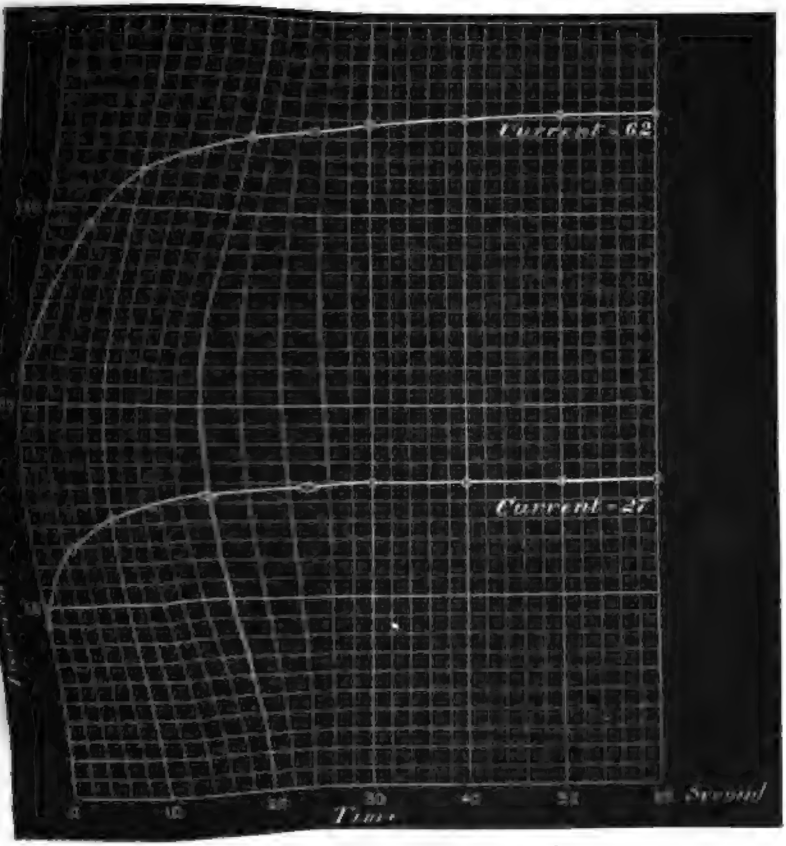
on p. 273, the equivalent effect of the compensating coil being added in each case to the actual magnetometer readings.

In fig. 2, curves are drawn to show the relation of the current to (*a*) the immediate magnetisation; (*b*) the magnetism after 5 seconds; and (*c*) the magnetism after 1 minute. The gradient of the curve (*a*) at and near the origin is the same as that of the corresponding curve in fig. 1, when allowance is made for difference of scales. In the present instance one division of current is 0·0013 of $\mathfrak{H}$, and one division of magnetism is 0·008 of $\mathfrak{I}$. The gradient begins to increase very sensibly when $\mathfrak{H}$ exceeds about 0·07.

Some of the results of Table I are also shown in fig. 3, which gives time curves of the growth of magnetism for the first two stages (currents 27 and 62). Similar curves for the other stages may readily be constructed from the table. It should be noticed that the time

rate of creeping is by no means excessively great in the first instants after contact is made; it is on this fact indeed that the practicability of the method depends.

FIG. 3.



Similar differences between the immediate and ultimate increments of magnetism present themselves when the magnetising force is increased step by step. In the following experiment the compensating coil was set so as to balance the immediate effect of a feeble magnetising current. Then such a current was applied, and the creeping up of the magnetism during 1 minute was observed. At the end of the minute the current was increased by a small step, and it was found that the compensation was still correct or very nearly so: in other words that the immediate effect of this small increase of magnetising force bore the same or very nearly the same proportion

to the increment of force as at the beginning of the process of magnetisation. The creep up of magnetism was again observed during a minute: then another small step up of the current was made, and so on. The compensation remained nearly correct for a number of steps, but as the process was continued up the curve of magnetisation, it became apparent that the immediate effect was *increasing*, in other words that there was under-compensation, and that the compensating coil would have to be moved a little forward if an exact balance was to be maintained. The results of this experiment are given below (Table II), and are exhibited in fig. 4. The magnetising current was increased from one to another of the successive values shown in the table at intervals of 1 minute in each case, by moderately quick movements of the sliding block in the liquid rheostat. The changes of magnetic force were therefore not quite sudden; each of them took perhaps a quarter of a second to complete.

Table II.

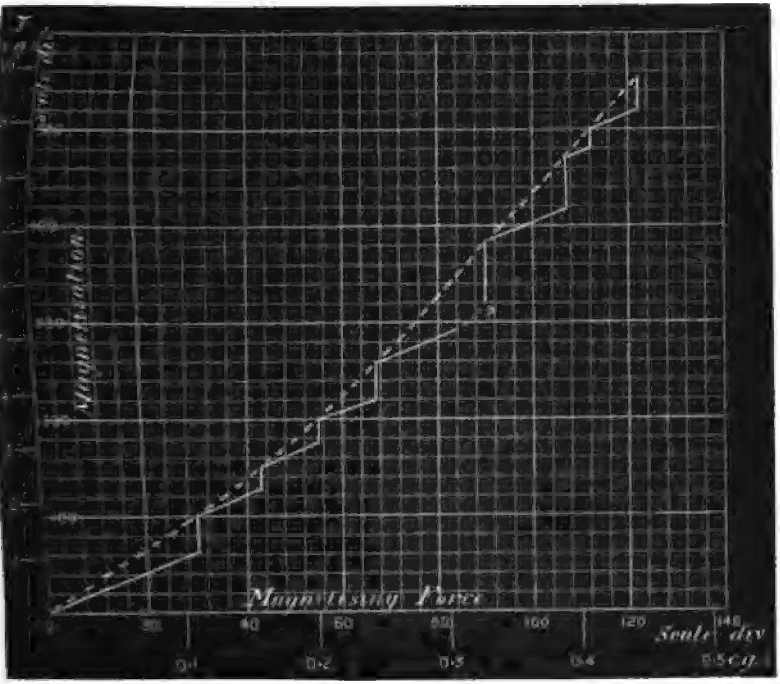
Magnetising current.		Immediate magnetic effect of step.	Additional increase of magnetism in 1 minute.	Total magnetism (after 1 minute).
Step.	Total.			
30	30	63	36	99
13	43	27	23	149
12	55	26	22	197
12	67	25	35	257
23	90	(49+)	(75−)	381
17	107	36	53	470
5	112	10	22	502
10	122	21	33	556

The step of 23 was too large to have its immediate effect balanced by the compensating coil in the position in which the coil was set. The magnetic effect of such a large step is conjecturally shown by the broken line marked (?) in fig. 4. It will be noticed that the points reached after 1 minute at each step lie well on a continuous curve, which is shown by a dotted line in the figure.

In Table II and fig. 4 one scale division of magnetising current is equivalent to 0·00362 c.g.s. units of magnetising force, and one scale division of the magnetometer is equivalent to 0·0177 c.g.s. units of $\mathfrak{I}$. The immediate value of $d\mathfrak{I}/d\mathfrak{H}$ is about 10, as before, and this applies approximately throughout the range of magnetism dealt with here, with a slight increase towards the upper end of the range.

Higher up in the curve of magnetisation, however, the immediate effect of a small quick increment of magnetic force is greater, though then (owing to the greater steepness of the curve $\mathfrak{I}$ and $\mathfrak{H}$) it bears

FIG. 4.



a smaller proportion to the ultimate effect. This is well shown in the following experiment (Table III and fig. 5).

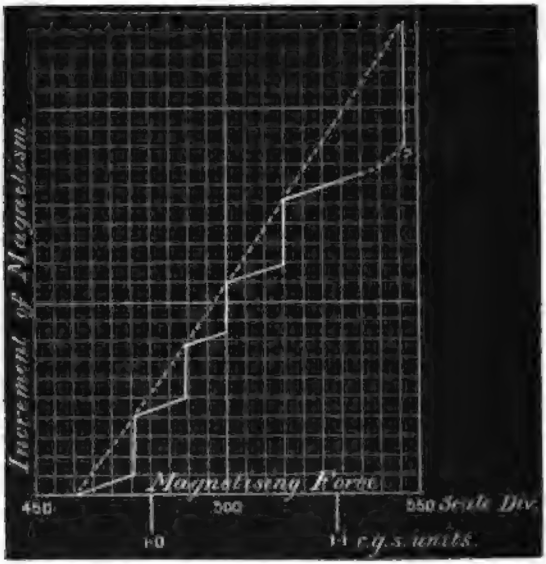
Table III.

Magnetising current.		Magnetising effect.	
Step.	Total.	Immediate.	Additional in 1 minute.
—	461	—	—
15	476	19	61
13	489	17	55
11	500	14	50
15	515	19	69
32	547	(41+)	(142–)

Here the last step was too large for perfect compensation. One scale division of current corresponds to 0·0021 c.g.s. units of magnetising force, and one scale division of the magnetometer corresponds to 0·022 of $\mathfrak{I}$. The immediate susceptibility to small increments of force, $d\mathfrak{I}/d\mathfrak{H}$, is now about 13. The magnetic viscosity is now so

great that this immediate effect is less than one-fourth of the whole change which the magnetisation has suffered by the time 1 minute has elapsed.

FIG. 5.

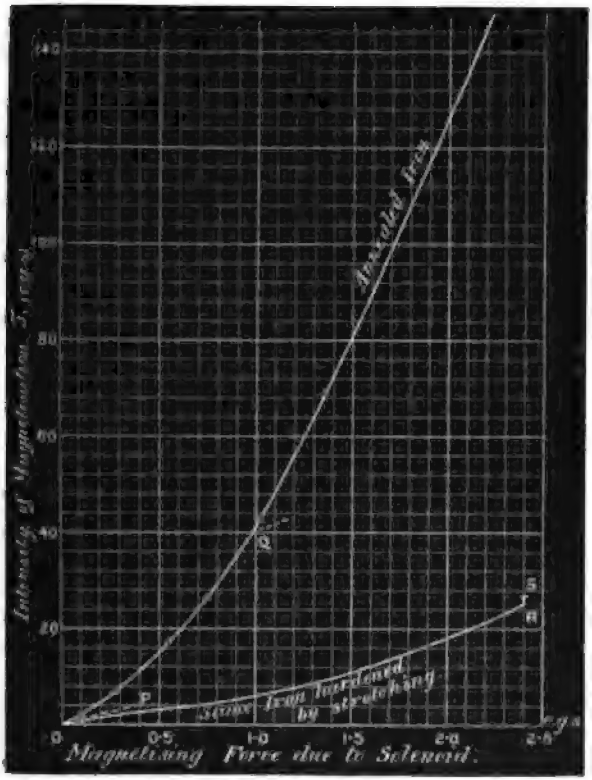


To show clearly the region in the curve of magnetisation at which the experiment of Table III and fig. 5 was made, a curve is drawn in fig. 6, showing, as the result of a separate experiment, the relation in absolute measure of the intensity of magnetism to the magnetising force produced by the solenoid. The region dealt with in Table III is at the place marked Q ($\mathfrak{I}$ = about 40 c.g.s.), and the dotted line drawn there shows the immediate value of $d\mathfrak{I}/d\mathfrak{H}$ after a 1-minute pause. The dotted line P shows the corresponding initial gradient, or immediate value of $d\mathfrak{I}/d\mathfrak{H}$, when there is no previous magnetisation.

Another step-by-step experiment of the same kind, made at a place higher up, where the magnetising force of the solenoid was about 4 c.g.s. and $\mathfrak{I}$ about 320, gave again about 1·3 for the immediate effect ($d\mathfrak{I}/d\mathfrak{H}$); and this was followed by a creeping to the extent of six or seven times the immediate effect.

The immediate magnetic effect of a small step is substantially the same whether the step is made quite suddenly by short-circuiting a resistance coil in the circuit of the magnetising solenoid, or comparatively gradually by means of the liquid slide, so that the process occupies a sensible fraction of a second, or even as much as a whole second.

FIG. 6.



However small the step is it appears to be followed by a creeping up of magnetism. I have been able to discover nothing which would correspond with the limit of perfect elasticity in straining a solid (if there be any true limit of elasticity), either in the initial part of the process of magnetisation, or after the prolonged application of a constant magnetising force.

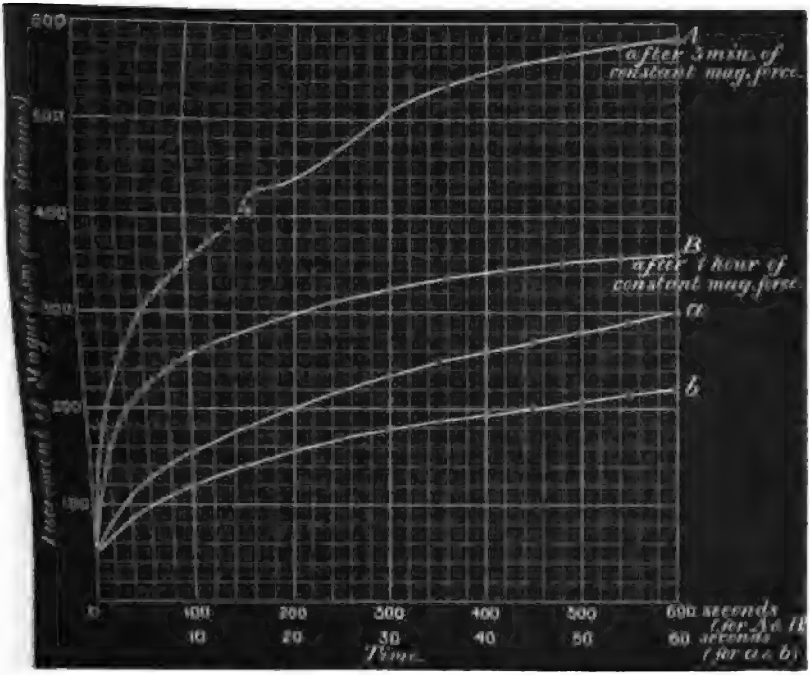
But the prolonged application of a constant magnetising force produces an effect which is a most interesting anologue of one effect of prolonged loading in a stretched wire. It is well known that when a load (sufficiently great to produce permanent set) is applied to a stretched iron wire, there ensues, with the lapse of time, not only a certain amount of supplementary viscous extension (the analogue of the magnetic creep) but also a quasi-hardening of the metal which becomes manifest when an addition is made to the load.* One effect

* *Cf.* 'Roy. Soc. Proc.,' No. 205, 1880, or 'Encycl. Brit.,' art. "Strength of Materials."

of this is that the wire responds with great sluggishness to the additional load, and this sluggishness is greater the longer has been the preceding interval during which the load has been maintained constant. To test whether, in like manner, the prolonged application of a constant magnetising force would produce what may be called magnetic hardening, I have made comparative observations of the time-rate of change of magnetism when a definite small increment of force is applied, the preceding force having been kept constant (*a*) for a short time and (*b*) for a long time. The result is to show that the process of magnetic creeping after a small step is much slower when the preceding force has been in action for a long time than when it has been in action for only a short time.

The following experiment illustrates this well. After raising the magnetising force to between 2 and 3 c.g.s. units, the compensating coil was adjusted to balance the immediate effect of a small increase of force, this increase being brought about by short-circuiting 1 ohm (out of many ohms) in the magnetising circuit. When the compensation had been adjusted, the iron was demagnetised by reversals, and the magnetising force was again gradually applied. When it reached the value of 2·54 c.g.s., a pause was made for 3 minutes, during which time this magnetising force of 2·54 remained constant. The resistance in the circuit of the magnetising current was then suddenly reduced by 1 ohm, which had the effect of raising the force to 2·60. The compensating coil prevented this step-up of magnetising force from having any instantaneous effect on the magnetometer; but creeping, of course, began at once, and the time-rate of creeping was observed during 10 minutes. Then the magnetising current was kept constant for 50 minutes more, making 1 hour in all, and a second step-up of magnetising force was effected by removing another ohm of resistance: the second step was very nearly equal to the first, and raised the force to 2·66 c.g.s. The time-rate of creeping which followed it was also observed during 10 minutes. The results are shown in fig. 7, where the curve A shows the growth of magnetism during 10 minutes when the step had been preceded by a 3-minute interval of constant force, and the curve B shows the growth of magnetism when a sensibly equal step was made, which had been preceded by a 1-hour interval of constant force. The times are in each case reckoned from the instant at which the step was made, and the increment of magnetism is in each case reckoned from the value reached just before the step was made. The immediate effect of each step (balanced by the coil) was equivalent to 51 scale divisions of the magnetometer. The creeping-up in 10 minutes was equal to no less than 531 scale divisions in the case of curve A, as against 320 in curve B. At the place marked with an asterisk in curve A, it happened that the laboratory door was slammed,

FIG. 7.



which shook the wire very perceptibly and caused a comparatively sudden increase of magnetism (indicated by the dotted part of the curve), after which the time-rate of creeping became specially slow for 1 or 2 minutes: finally, however, the rate appeared to recover from this disturbance. The curves *a* and *b* of fig. 7 are the first parts of A and B drawn to a ten-fold coarser scale of times.

In confirmation of the above, another experiment was made in which the magnetic force was increased by three successive small and very nearly equal steps. The first step was made after 5 minutes of constant force, the second after 1 hour of constant force, and the third again after 5 minutes of constant force. Time-curves of the growth of magnetism were drawn in all three cases. The first and third curves were not far from coincident; but the second curve lay very much below them, as B lies below A.

In the experiments to which figs. 4, 5, and 7 relate, the increment of magnetic force whose effects were measured was preceded by increasing magnetic forces: in other words, it was a step-up from a point on the *up* curve of magnetisation. I have also examined the effect of a small *step-down* from a point on the up curve—that is to say, a small decrement of previously increasing force—and find, as

might perhaps be anticipated from what we knew about static hysteresis, that the immediate effect ($d\mathfrak{I}/d\mathfrak{H}$) of a step-down is decidedly less than the immediate effect of a step-up. When the compensating coil had been adjusted to balance the first effect of a step-up, it was found to give over-compensation for a step-down.

Another process has been examined, namely, the alternation of a step-up with step-down, many times repeated. After the magnetising current had been raised to a certain value, it was periodically altered through a definite narrow range by alternately putting in and pulling out the short-circuit plug of a small resistance coil in the main circuit, or by making and breaking a feeble circuit in a second solenoid wound over the first. It was only when this process had been repeated many times that the magnetic effects of the small changes of $\mathfrak{H}$ became approximately cyclic; the early cycles were associated with a progressive rise in the intensity of magnetism. But when a nearly cyclic state was reached, the compensating coil could be adjusted to balance the immediate effects of $+\delta\mathfrak{H}$ or $-\delta\mathfrak{H}$, and the same adjustment of course served to balance either.

Tested in this way the gradient $d\mathfrak{I}/d\mathfrak{H}$ (for the immediate effect of $\delta\mathfrak{H}$ after many small $+$ and $-$ steps) has of course a lower value than the gradient which is found when $\mathfrak{H}$ is first raised to $\mathfrak{H} + \delta\mathfrak{H}$. The latter, as we have seen, is greater when the magnetisation is moderately strong than when there is little or none. The former is nearly constant throughout a wide range of $\mathfrak{I}$; its value is approximately the same as at the initial part of the magnetisation curve—namely 10—until the region of saturation is approached, when it becomes distinctly less.*

The periodic changes of magnetism which are brought about by successive small increments and decrements of $\mathfrak{H}$ exhibit a lagging and creeping up and down precisely similar to that which has been illustrated in fig. 1. That figure may serve to show in a general way the relation of the change of $\mathfrak{I}$ to the change of $\mathfrak{H}$, when at any place in the curve a very small increment $\delta\mathfrak{H}$ has been applied and removed often enough to establish a cyclic *régime*. I have not made any full examination of the variation which under these conditions the gradient $d\mathfrak{I}/d\mathfrak{H}$ suffers when the magnetism on which the small cycle if superposed is gradually pushed up towards saturation, nor of the proportion which the subsequent creeping up or down bears to that part of the change of $\mathfrak{I}$ which occurs immediately on the application or removal of $\delta\mathfrak{H}$. The creeping which follows each repeated application and removal of $\delta\mathfrak{H}$ is certainly much reduced when the iron approaches saturation; but the immediate effect is also reduced, and so far as may be judged by rather rough determinations, it appears

* *Cf.* Lord Rayleigh, *loc. cit.*, on the approximate constancy of the *static* gradient $d\mathfrak{I}/d\mathfrak{H}$.

that the proportion of creeping to immediate effect is much the same with high as with low magnetisation.

One may refer, in this connexion, to the energy which is dissipated through hysteresis, in performing a small cycle by alternately applying and removing a very small force $\delta\mathfrak{H}$. The action is the same in kind whether there is or is not additional magnetisation. The energy dissipated in each cycle is $-\int \mathfrak{J} d\mathfrak{H}$, and vanishes when the increment and decrement of $\mathfrak{J}$ go on *pari passu* with the increment and decrement of $\mathfrak{H}$.

Consider now fig. 1. When the repeated cyclic changes of $\mathfrak{H}$ are indefinitely rapid and go on without pause, so that creeping has not time to occur, a single straight (or sensibly straight) line such as OA represents the relation of the change of magnetism to the (very small) change of magnetising force, during both increment and decrement. The rapidity of the action prevents any loop from being formed, and there is consequently no sensible dissipation of energy through hysteresis. This state of things is perhaps nearly realised in the case of a vibrating telephone diaphragm, or, in regard to circumferential magnetisation, by an iron conducting wire in a telephone circuit. Again, let the cycle be performed indefinitely slowly. In that case the magnetism, at every stage of the cycle, creeps up or down to a steady value. A sensibly straight line, such as OB, represents the relation of $\mathfrak{J}$ to $\mathfrak{H}$ during both increment and decrement; and there is again no dissipation of energy. But with any frequency of alternation lying between these extremes of infinitely fast and infinitely slow, a loop will be formed, since the creeping will take effect most considerably at and near the ends of the range (the time-rate of change of $\mathfrak{H}$ being least there), and there will be dissipation of energy. When the limits and mode of variation of $\mathfrak{H}$ are specified, there must be sóme particular frequency which will make the energy dissipated per cycle a maximum.

The phenomena described in the paper have been reproduced in several specimens of annealed iron wire, of course with quantitative differences. As to the amount of magnetic creeping much depends on the annealing of the specimen. Another piece of iron wire cut from the same bundle as the piece with which these experiments were made, and annealed at another time, showed almost exactly the same susceptibility to magnetism as the first piece, so far as immediate effect went; but in it the subsequent creeping up was decidedly less (in the proportion of about 4 to 5).

When the iron is hardened by mechanical strain the phenomena of creeping vanish almost completely. A specimen from the same bundle was annealed, and showed much creeping. It was then put in the testing machine and pulled until it took a set of 1 or 2 mm.

in a length of 40 cm. or so. It was then examined magnetically as before, and scarcely a trace of creeping could be observed when a feeble magnetising force was applied. When the compensating coil was properly adjusted the making or breaking of the magnetising current caused no more than a slight momentary quiver of the magnetometer needle, followed by no measurable drifting, although the whole magnetic effect (compensated by the coil) was equivalent to a hundred or more scale divisions. When a magnetising force of as much as 0·6 c.g.s. unit was suddenly applied, the amount of creeping, if there was any, was certainly less than 1 per cent. of the immediate effect. With values of $\mathfrak{H}$ higher than this it became possible to detect creep with certainty. The following notes relate to this wire:—

Magnetising force suddenly applied. (c.g.s.)	Immediate value of $\mathfrak{I}$ (c.g.s.).	
0·75	4·49	
1·28	8·42, crept in 1 min. to	8·58.
2·40	25·5 „	26·4.

These forces were in each case applied to this wire in a neutral state. Another trial of the same, with feebler forces, gave 5·3 as the value of $d\mathfrak{I}/d\mathfrak{H}$ for the immediate effect of a very small force, applied when the iron was demagnetised. The same quantity in the annealed specimen was, as has been said, about 10. In fig. 6 the relation of $\mathfrak{I}$ (immediate) to $\mathfrak{H}$ as stated above, is represented by the curve OR; the creeping up at the last point is RS.

In speaking of soft iron it has been shown that the effects of creeping are most marked when a small addition $\delta\mathfrak{H}$ is made to a previously increasing force $\mathfrak{H}$. In instances quoted above, the creeping up in 1 min. has under those conditions been many times greater than the immediate effect of $\delta\mathfrak{H}$.

By way of putting the specimen of hardened iron to the same test, I have applied a magnetic force of 1·46 and raised it by a small step to 1·49. The immediate effect of this step (which was balanced by the compensating coil) was equivalent to twenty-two scale divisions of the magnetometer, and this was followed during 1 minute by a creeping equal to six scale divisions. In itself this creeping is considerable, but compared with the corresponding creeping in soft iron it is extremely small.

Pieces of steel (containing a good deal of carbon) have also been examined, with the result that whether the steel be annealed or in its commercial temper the phenomenon of creeping is even less visible than in hardened iron. With annealed steel, a force which produced an immediate (compensated) magnetic effect equal to 124 scale divisions caused barely a single scale division of creeping. With a stronger current, giving an immediate magnetism of 340, the sub-

sequent creeping was 3. In steel and in hard iron the creeping seemed to be completed in a few seconds after the institution of the magnetising current. The steel specimen, like the iron, had a diameter of rather more than 4 mm. Its susceptibility (annealed) was considerably less than that of the iron in the hard state.

It is scarcely necessary to observe that the protracted and extensive creeping or magnetic "nachwirkung" in soft iron which these experiments illustrate cannot be ascribed to the subsidence of the circumferential currents which are generated by the imposition of longitudinal magnetic force. The creeping is equally conspicuous whether the magnetic force is suddenly or gradually imposed. Lord Rayleigh has shown that circumferential currents started and left to themselves will subside to e^{-1} of their initial magnitude in the time

$$\tau = \frac{4\pi\mu a^2}{(2{\cdot}404)^2\rho},$$

where a is the radius of the cylinder, μ its permeability, and ρ its specific resistance.* In the present instance, taking the case of the annealed iron rod, $a = 0{\cdot}202$, $\mu = 125$, $\rho = 9827$ (Everett), and τ is less than $\frac{1}{1000}$ of a second. The subsidence would be practically complete in a small fraction of a second: but the creeping persists during many seconds and even minutes with no excessive change of rate. Again, comparing soft iron with hard iron, in which μ is less and ρ is greater, the values of τ will differ, but not by any means so much as to correspond with the very wide difference in magnetic lag.

In view of this it is puzzling to find that the diameter of the rod experimented upon has a most important influence on the magnetic lag.

In testing various samples of soft iron wire, most of which were of less diameter than the piece used in the above experiments, I noticed that the phenomena of creeping were less marked in the smaller rods. I then tried a bundle of nine very soft annealed iron wires, which were bound together with fine copper wire, and formed a core of about the same length and aggregate diameter as that of the solid rod formerly used. With this bundle there was some creeping, but very little in comparison with what was observed in the solid rod, as the following notes show:—

Bundle of nine soft Iron Wires.

Magnetic force $\mathfrak{H}$ suddenly applied (c.g.s.).	Magnetometer deflections. Immediate (balanced by compensating coil).	Subsequent creeping in 1 min.	Total in 1 min.
0·052	17	2	19
0·147	52	9	61

* 'Brit. Assoc. Report,' 1882, p. 446.

Finally, another bundle was built up, consisting of a much larger number of fine annealed iron wires. With this the creeping was almost insensible.

It may be that the comparative absence of magnetic creeping, or "nachwirkung," in these last experiments is to be ascribed to the quickness with which the process of creeping completes itself in a finely divided mass of iron: in other words, that the process is practically complete in a time much shorter than the period of the magnetometer needle. The marked difference in effect between a solid core (a single thick wire) of soft iron and a laminated core (a bundle of fine wires) of the same material, suggests that in the former much more than in the latter the process of creeping is retarded by the eddy currents which are set up by those molecular movements in which the process itself consists.

[July 11th.—In seeking an explanation of the difference in behaviour it may be worth while to bear in mind that there is probably a considerable difference in molecular structure between a solid core and a laminated core of iron. If we accept the view that the magnetically neutral state is due to the molecular magnets forming closed rings, these rings will for the most part be closed within the limits of the separate constituent pieces of the laminated core, whereas in the solid core they may be much larger, their dimensions being limited only by those of the core itself.]

I have received very valuable help in these experiments from two students, Mr. David Low and Mr. William Frew, who have prosecuted a troublesome research with much patience and zeal.

III. "Note on the Thermo-electric Position of Platinoid." By J. T. Bottomley, M.A., F.R.S., and A. Tanakadate, *Rigakusi*. Received June 13, 1889.

In carrying out a series of experiments on radiation of heat by solid bodies, an investigation to which one of the present writers has for some time past devoted considerable attention, it became necessary, for a purpose which need not here be detailed, to select a thermo-electric pair of metals, of which one metal is essentially platinum, as it passes through glass. Various pairs were considered, and some trials were made; and it was finally determined to make use of platinum and platinoid. The latter metal is an alloy whose electrical and mechanical properties were investigated some years ago by one of the present writers;* and since that time it has

* J. T. Bottomley, 'Roy. Soc. Proc.,' 1885.

assumed considerable importance in the construction of electrical instruments and resistance coils. Partly on this account, and partly from present requirements, it became both interesting and necessary to determine the thermo-electric constants for a specimen of this alloy.

Platinoid is in composition very similar to German silver. In the manufacture of the alloy, however, phosphide of tungsten is employed; and although an exceedingly minute quantity of metallic tungsten remains in the alloy, yet the properties of the substance are in many respects remarkable. The metal is capable of being polished so as to be almost as beautiful as silver in appearance, having only a slightly darker and more steel-like colour; and when it has been polished it remains absolutely untarnished, even in the atmosphere of a large town, for years at any rate. It has very remarkable properties as to electric resistance. It possesses a very high resistance, while at the same time it has a much lower temperature variation of electric resistance than any other known metal or alloy. It has also, as Sir William Thomson has found, very excellent elastic qualities.

Although it is not proposed to use the platinoid with any metal other than platinum in the investigation on thermal radiation above referred to, it nevertheless seemed advisable, when these experiments were being undertaken, to determine its position with respect to some other metals. It was accordingly tried as a pair with platinum, iron, aluminium, and with two specimens of copper.

A low-resistance Thomson's reflecting galvanometer was specially prepared for the purpose of these experiments. The mirror was a plane parallel mirror of very excellent quality by Steinheil of Munich. Its deflections were observed by means of a telescope with cross-wires and scale, instead of by a lamp and scale. To avoid any influence of the suspending fibre (which even though of single cocoon silk fibre does with short fibres give an appreciable torsional resistance) the mirror was suspended by spider line. The suspending of a mirror, weighing with its magnet 0·2 gram, by a single spider line is a matter of some nicety and difficulty; but when it has been accomplished the result is so thoroughly satisfactory that it is easily admitted to be well worth a morning's labour.

To make the suspension two small pieces of very thin bristle or of hard-spun silk fibre or split horsehair are attached to the ends of a suitable length of spider line recently spun by a good large* spider. By means of these attachments, which are easily seen, the spider line can be handled. It is then brought over the galvanometer mirror; and great assistance is experienced in these operations, and in operations with single silk fibres, by performing them on the top of a piece of looking-glass laid on the table. The illumination from beneath of

* The body about as large as a pea.

the fibres makes it easy to do with these fine filaments that which is otherwise scarcely possible. The fibre is attached to the galvanometer mirror with the smallest possible speck of shellac varnish, the greatest care being taken not to varnish any part of the spider line. When the varnish has dried, the mirror can be lifted up by the spider line; caution being used at the moment of raising the one mirror off the surface of the other on account of the vacuum which is liable to be formed at the moment of separation. The mirror should be allowed to hang on the fibre inside a glass beaker for twenty-four hours at least, as the spider line stretches considerably for some time after the weight comes on it. A spider line which will carry a galvanometer mirror and magnet weighing 0·2 gram may have, according to an estimate made by one of the present writers, about $\frac{1}{700}$ of the torsional rigidity of a single cocoon silk fibre.

For the heating of the junctions, a number of glass vessels were blown, resembling the flasks, with neck and condensing tube, used for fractional distillation, but with the condensing tube projecting upwards into the air, so that the steam of a liquid boiling in the flask runs back into the flask on being condensed. Into the shorter neck of the flask was introduced a cork, which carried the thermo-junction and a mercurial thermometer; the thermo-junction being loosely bound to the bulb of the thermometer, or, at any rate, kept in close contact with the middle part of the thermometer-bulb. The cool junction was bound to the bulb of a second thermometer, which dipped into a vessel containing water at the temperature of the laboratory. The water was kept thoroughly stirred from top to bottom by a properly arranged stirrer.

In the heating flasks the vapours of the following liquids were used: alcohol, water, chlorobenzol, aniline, methyl salicylate, and bromobenzol.* The liquids were boiled vigorously, and the temperatures of the vapours were determined by means of the mercurial thermometer. Both the mercurial thermometers were compared directly with the air thermometer.† The obtaining of a set of points of temperature by this means was very satisfactory in every case except that of the liquid of highest boiling point, bromobenzol. In this case a curious phenomenon was observed.‡ In spite of the fact that the vapour of the substance was rushing strongly into the condensing tube and, indeed, out into the open air, at an elevation of 2 feet above the surface of the liquid it was found exceedingly difficult to keep the temperature of the various parts of the boiling flask anything like uniform. The vapour formed itself into layers of different temperatures, the parts of the flask nearest the surface of the liquid being the hottest. At

* Ramsay and Young, 'Chem. Soc. Journ. (Trans.),' 1885.

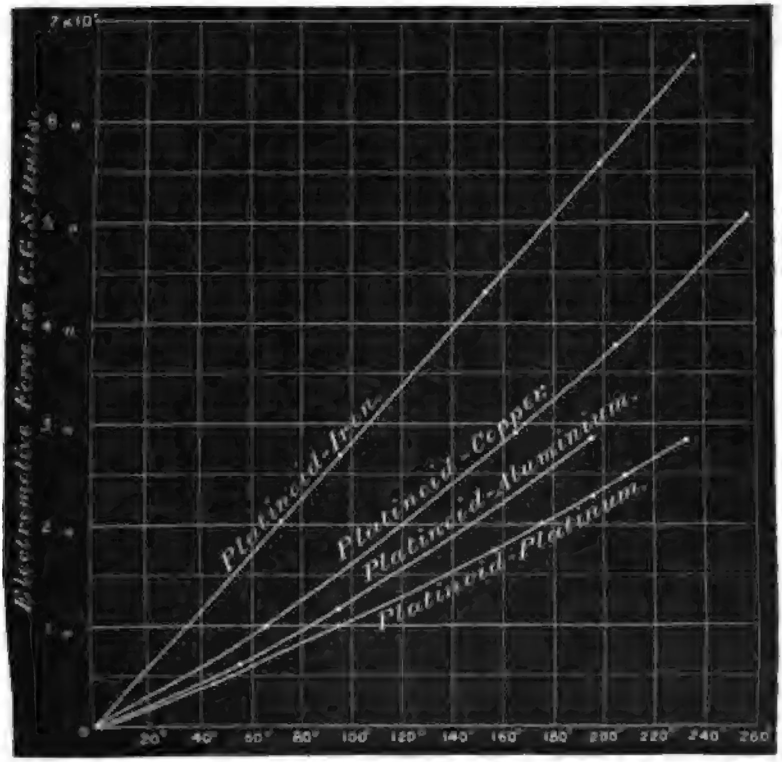
† J. T. Bottomley, 'Phil. Mag.,' August, 1888.

‡ Perhaps due to want of purity of the substance.

a height of $2\frac{1}{2}$ inches above the surface of the liquid the temperature was often found to be as much as 8° or 10° C. cooler than it was just above the surface. The difficulty could, to a certain extent, be overcome by putting a cloak of fine flexible wire gauze all round the upper part of the flask; but the greatest watchfulness was needed to avoid mistakes.

In order to reduce the results obtained from the readings of the galvanometer to absolute electromagnetic measure, a carefully prepared standard Daniell's cell was kept with its current always flowing through a known high resistance; and from time to time the galvanometer which was being used was thrown into the circuit, and the value of the galvanometer deflection determined. The electromotive force of the Daniell's cell was valued at 1·072 volts.

The results obtained are shown in the accompanying curves and tables.



In the curves the electromotive forces are shown as ordinates, the differences of temperature between the hot and cold junctions being indicated on the axis of abscissas. The electromotive forces are given

in C.G.S. units, and must be divided by 10^8 if it be desired to reduce them to volts. The differences of temperatures are given in centigrade degrees. The direction of the current in each of the cases represented, is from platinoid to the second metal of the pair through the hot junction.

Table I shows, in the way now well known,* the multiplier, at any temperature centigrade, which must be used, as factor with the difference of temperatures between the hot and cold junctions, in order to calculate the electromotive force in C.G.S. units. The algebraic sign corresponds with that used by Tait, and now adopted by Everett ('Units and Constants,' 2nd Edition, 1886).

Table I.

Platinoid-platinum..............	$-925-1{\cdot}16\times t$.
Platinoid-aluminium	$-985-4{\cdot}31\times t$.
Platinoid-iron	$-2916+0{\cdot}86\times t$.
Platinoid-copper (A)	$-1246-5{\cdot}44\times t$.
Platinoid-copper (B)	$-1294-4{\cdot}88\times t$.

Combining the results of Table I with those of Tait, reduced by Everett, we obtain the thermo-electric distance of platinoid from lead, taken as zero, at various temperatures centigrade. If any one of the wires platinum, aluminium, iron, or copper used by us, were identical with the wire of the same name used by Professor Tait, we should be able to deduce with exactness the distance of our platinoid wire from his lead wire. That, however, was not the case; and each of the secondary wires used by us gives us, as it were, a different result. Thus we have :—

Table II.

			Platinoid to lead.
From experiment	with	platinum	$-986-2{\cdot}26\times t$
,,	,,	aluminium	$-1062-3{\cdot}92\times t$
,,	,,	iron	$-1182-4{\cdot}01\times t$
,,	,,	copper (A)	$-1110-4{\cdot}49\times t$
,,	,,	copper (B)	$-1158-3{\cdot}93\times t$

Taking the mean of all of these, with the exception of the result for platinum, which we omit because different specimens of platinum are well known to differ thermo-electrically enormously among themselves, we obtain for the thermo-electric distance of platinoid from Professor Tait's lead wire $-1128-4{\cdot}1\times t$.

This result enables us to place platinoid in Tait's thermo-electric diagram. Its line is nearly parallel to those of palladium and German

* Tait, 'Edinburgh Roy. Soc. Trans.,' vol. 27, 1873, and Everett's 'Units and Constants,' 2nd Edition, 1886,—"Thermoelectricity."

silver, and slightly above the latter. It is, however, to be remembered that, in all probability, different specimens of platinoid alloy would give results differing considerably from that quoted above.

Appendix. By A. TANAKADATE.

The following experiment on the torsional rigidity of spider line was carried out in the Physical Laboratory of the Imperial University of Japan, in 1884, and a notice of it was published in vol. 2 of *Rigakukyokwai Tassi* ('Proceedings of the Science Society') of that year in Japanese. It has not hitherto been described in English; and the absolute determination as referred to below by Mr. T. Gray of the rigidity of silk fibre makes an estimate of the rigidity of spider line possible.

The determination of the torsional rigidity was a relative one, and the experiment essentially consisted in finding the deflection of a small magnet due to a given twist of the suspending fibre: the magnet being placed in the earth's magnetic field (0·3 C.G.S.). The deflection was observed by the usual method of the reflected image of a fine wire stretched before a lamp.

The mirror magnet was first hung by a silk fibre of 31 cm. length, and placed in the usual way. The distance of scale from the mirror was 95 cm. When the torsion head of the magnetometer was turned through one complete revolution (2π) in either direction from zero, the image of the reflected wire was displaced through 8 mm. either way, or $8/2 \times 95 = 0{\cdot}0042$ radians, or 864″.

The silk fibre was now detached from the magnet, and a spider's line (newly spun) was attached in its stead. The length was 28 cm, the magnetometer was put into its place, and the torsion head was turned as before, but no appreciable deflection could be observed, even when the torsion head was turned through ten complete turns (20π). It was suspected then that the mirror might have been caught against the sides of its case; a close inspection, however, showed that it was quite free. The fibre was then shortened to 2·3 cm. (about one-twelfth its previous length), and the experiment was repeated. Ten complete turns of the torsion head gave a deflection of 1·5 mm.; or $\cdot 15/2 \times 95 = 0{\cdot}00079$ radians $= 16{\cdot}3''$ per turn.

In order to compare these deflections with each other, each deflection was reduced to that which would be given by a fibre of 1 cm. in length, by multiplying the deflections by the length of the fibre used. Thus, corresponding to the twist of one turn of the torsion head in a fibre of 1 cm. long, we have:—

For silk fibre	$864{\cdot}0'' \times 31 = 26800''$
For spider line	$16{\cdot}3'' \times 2{\cdot}3 = 37{\cdot}5''$

From this we get the ratio of the torsional rigidity of the spider line to that of the silk fibre to be 1 : 710.

The diameters of the fibres were microscopically measured, and gave the following values:—

Silk fibre....................	0·00091 cm.
Spider line..................	0·00028 ,,

If the elastic qualities of these fibres were the same, the ratio of the torsional rigidity would have come out $(28)^4 : (91)^4$, or 1 : 112; and hence the torsional rigidity of spider line is less than one-sixth of that of silk fibre of the same thickness.

The above result gives us only a relative value of the rigidities between the two fibres. If we take the mean value of the torsional rigidity of silk fibre to be 0·0012 C.G.S. on a length of 1 centimetre (not per square centimetre), as found by Mr. T. Gray,* the torsional rigidity of the spider fibre of the above experiment will be $\frac{0\cdot0012}{710} = 0\cdot000002$ C.G.S., the mode of reckoning being the same.

Mr. Gray's silk fibre may have had a slightly higher rigidity, as he states that it was boiled in water, while the fibre of the experiment just described was taken from those boiled in dilute potash water, as is the usual practice of preparing "mawata," which is a very soft kind of silk.

IV. "Specific Inductive Capacity of Dielectrics when acted on by very rapidly alternating Electric Forces." By J. J. THOMSON, M.A., F.R.S., Cavendish Professor of Physics, Cambridge. Received June 17, 1889.

The researches of Dr. John Hopkinson have shown that in some dielectrics, of which the most conspicuous example is glass, the refractive index is not, as it ought to be on Maxwell's theory, equal to the square root of the specific inductive capacity, when the latter is measured for steady forces, or such as are reversed only a few thousand times a second. It is therefore desirable to measure the inductive capacity under circumstances which approach as nearly as possible to those which, according to Maxwell's theory, occur when light passes through a dielectric. This will be when the forces are reversed as rapidly as possible. In the following experiments the forces were reversed about 25,000,000 times per second.

The method consists in measuring the wave-length of the electrical vibrations given out by a condenser whose plates are in electrical connexion. If C is the capacity in electrostatic measure of the con-

* 'Phil. Mag.,' 1887.

denser, L the coefficient of self-induction in electromagnetic measure of the circuit connecting the plates of the condenser, the wave-length, if it is long compared with the length of this circuit, equals $2\pi\sqrt{(LC)}$. Thus, if we can measure the wave-length of the vibrations executed by such a system, we can find the specific inductive capacity of a dielectric. For, if we determine the wave-length of the system first when the plates of the condenser are separated by air, and then when they are separated by a slab of the dielectric whose specific inductive capacity we wish to measure, the ratio of the squares of the wave-lengths will be the ratio of the capacities of the condenser in the two cases, and if we know this ratio we can deduce the specific inductive capacity of the dielectric interposed between the plates.

FIG. 1.

The arrangement of the experiment was as follows:—

The condenser consisted of two circular zinc plates, AB, CD, 30 cm. in diameter; these were supported on an insulating stand, and the distance between them could be altered at pleasure. To these plates wires, EF, GH, each about 25 cm. in length, terminating in the highly polished balls, F, H, were attached. The plates were also connected with the poles P, Q, of an induction coil, and when this was in action a succession of sparks passed between the balls F and H. The periodic distributions of electricity thus produced over the plates sent electrical waves down two insulated wires, each about 20 metres in length, attached to the small zinc plates, L and M, placed close to the plates of the condenser.

The wave-length of the vibrations transmitted along the wire was determined by the method I described in a former paper ("Note on the Effect produced by Conductors in the Neighbourhood of a Wire on

FIG. 2.

the Rate of Propagation of Electrical Disturbances along it," 'Roy. Soc. Proc.,' vol. 46, p. 1). Two wires RS, VW, of equal length, had the ends S and W fastened to the poles of a spark micrometer, while the other ends, R, V, could slide along the wires LT, MU respectively. At first R was placed at T, and V was moved until the sparks in the micrometer were as small as possible; suppose that α was the position of V when this was the case. T and α will be at the same potential. The end V was now kept fixed at α, and R moved until the sparks again became as faint as possible; suppose that β was the position of R when this was the case, then β and α, and therefore β and T, are at the same potential; so that, since T is a place of maximum potential, βT equals a wave-length.

By starting from β and proceeding further up the wire we can get another determination of the wave-length.

Since from the nature of the case other conductors besides the two disks were in the field, the capacity of the condenser was in excess of the value given by the formula $S/4\pi t$, where S is the area of one of the plates and t the distance between them; but this is the only part of the capacity which is increased when the slab of dielectric is interposed between the plates. The capacity when the disks were 2 cm. apart was determined by the tuning-fork method given in Maxwell's "Electricity and Magnetism," vol. 2, p. 385, and was found to be

40 in electrostatic units; the formula $S/4\pi t$ would, where $S = \pi \times 15^2$ and $t = 2$, give 28, so that of the 40 units of capacity, 28 are due to the two disks and 12 to the presence of the other conductors. This was verified by determining by the tuning-fork method the capacity of the condenser when the distance between the disks had a series of values.

When the distance between the disks was 2 cm., the mean of several determinations of the wave-length along the wire was 8·25 metres. The value calculated by the formula $2\pi\sqrt{(LC)}$, where $C = 40$ and $L = 2l\left(\log \frac{8l}{\pi d} - 2\right)$, where l = length of circuit (supposed circular) = 50 cm., and d the diameter of the wire = 0·3 cm., is 8 metres. When the plates were separated by pieces of plate glass 2 cm. thick, the wave-length was 11·75 metres. Thus, if K is the specific inductive capacity of the glass,

$$\frac{11\cdot75}{8\cdot25} = \sqrt{\frac{28K+12}{40}},$$

$$K = 2\cdot7, \qquad \text{and} \quad \sqrt{K} = 1\cdot65.$$

The determination of the specific inductive capacity of the glass by the tuning-fork method was difficult, owing to electric absorption; the values for K obtained in this way varied between 9 and 11. We see, therefore, that for vibrations whose frequency is $3 \times 10^{10}/11\cdot75 \times 10^2$, or 25,000,000 per second, the specific inductive capacity is very nearly equal to the square of the refractive index, and is very much less than the value for slow rates of reversals. The discrepancy is probably due to the cause which produces the phenomenon of anomalous dispersion in some substances, and indicates the existence of molecular vibrations having a period slower than 25.000,000 per second. The behaviour of the glass under electrical oscillations of the critical period would form a very interesting subject of investigation.

The specific inductive capacity of ebonite was determined in a similar way; the wave-lengths, when the plates were separated by air and ebonite respectively, were 8·5 metres and 10·75 metres, giving as the specific inductive capacity of ebonite 1·9. The value determined by the tuning-fork method was 2·1.

The specific inductive capacity of a plate made of melted stick sulphur was also tried: the wave-length without the sulphur was 8·25, with it 11·5, giving as the specific inductive capacity of sulphur 2·4. The value determined by the tuning-fork method was 2·27. Thus, for ebonite and sulphur the values determined by the two methods agree as well as could be expected, while for glass the results are altogether different.

V. "A new Form of Gas Battery." By LUDWIG MOND and CARL LANGER. Communicated by LORD RAYLEIGH, Sec. R.S. Received June 13, 1889.

In January, 1839, now over fifty years ago, Mr. (now Lord Justice) Grove published the first notice* of his startling discovery—the gas battery. This he followed up in 1842, 1843, and 1845 by three important papers†, two of which were read before this Society.

Since that time very little attention has been given by investigators to the subject. Papers by Schönbein,‡ De la Rive,§ Matteucci,|| Beetz,¶ Gaugain,** Morley,†† Peirce,‡‡ Lord Rayleigh,§§ Figuier,|||| and Kendall,¶¶ and a few patents describing ingenious but impracticable suggestions for improved gas batteries, comprise the principal contributions to the subject.

This is the more surprising as Grove had published a large number of experiments leading, amongst other important results, to a complete list of the voltaic relations of gases to each other and to other substances, and had pointed out in his lucid manner the great scientific interest attaching to the gas battery, which forms the simplest instrument for generating electricity, possesses remarkable constancy of E.M.F., and "exhibits such a beautiful example of the correlation of natural forces."

Grove states that he never thought of the gas battery as a practical means of generating voltaic power, but, nevertheless, he indicates clearly in what directions improvements with this object should be attempted, viz., by extending as much as possible the surface of contact between the gases, the absorbent and the electrolyte.

We have been engaged for several years with investigations on gas batteries, which fully corroborate Grove's view, but show that he, as

* Grove, 'Phil. Mag.,' vol. 14, 1839, p. 129.

† Grove, 'Phil. Mag.,' vol. 21, 1842, p. 417; 'Roy. Soc. Proc.,' vol. 4, 1843, p. 463; vol. 5, 1845, p. 557.

‡ Schönbein, 'Poggendorff, Annalen,' vol. 56, 1842, pp. 135 and 235; vol. 58, 1843, 361; vol. 62, 1844, 220; vol. 74, 1849, 244.

§ De la Rive, 'Arch. d'Électric.,' vol. 3, 1843, p. 525.

|| Matteucci, 'Comptes Rendus,' vol. 16, 1843, p. 846.

¶ Beetz, 'Poggendorff, Annalen,' vol. 77, 1849, p. 505; vol. 90, 1853, p. 42; vol. 132, 1867, p. 460; 'Wiedemann, Annalen,' vol. 5, 1878, p. 1; 'Phil. Mag.,' vol. 7, 1879, p. 1.

** Gaugain, 'Comptes Rendus,' vol. 64, 1867, p. 364.

†† Morley, 'Phil. Mag.,' vol. 5, 1878, p. 272.

‡‡ Peirce, 'Wiedemann, Annalen,' vol. 8, 1879, p. 98.

§§ Rayleigh, 'Cambridge Phil. Soc. Proc.,' vol. 4, 1882, p. 198.

|||| Figuier, 'Comptes Rendus,' vol. 98, 1884, p. 1575.

¶¶ Kendall, 'Roy. Soc. Proc.,' vol. 36, 1884, p. 208.

well as later investigators, overlook one important point, viz., the necessity of maintaining the condensing power of the absorbent unimpaired. We found that platinum black, the most suitable absorbent for gas batteries, loses its condensing power almost completely as soon as it gets wet, and that it is therefore necessary for our purpose to keep it comparatively dry. All attempts to attain this with various constructions of the gas battery involving the use of a liquid electrolyte failed. We have only succeeded by using an electrolyte in a quasi-solid form, viz., soaked up by a porous non-conducting material, in a similar way as has been done in the so-called dry piles and batteries.

In order to procure as large a contact as possible between the gases, the electrolyte and the absorbent, and at the same time to obtain the greatest possible duty out of a given quantity of the latter, we have adopted the following construction:—

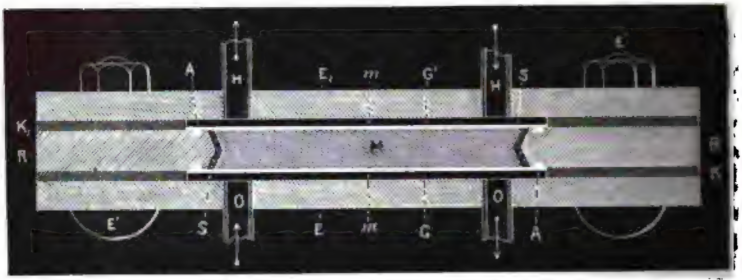
A diaphragm of a porous non-conducting substance, such as plaster of Paris, earthenware, asbestos, pasteboard, &c., is impregnated by dilute sulphuric acid or another electrolyte, and is covered on both sides with thin perforated leaf of platinum or gold and with a thin film of platinum black. The platinum or gold leaf, which serves as conductor for the generated electricity (the platinum black being a very bad conductor), is placed in contact at small intervals with strips of lead or other good conductor in order to reduce the internal resistance of the battery to a minimum. In place of the platinum or gold leaf, fine wire gauze of the same metal or of carbon may be used.

The diaphragms so prepared are placed side by side or one above the other, with non-conducting frames of pasteboard, wood, india-rubber, &c., intervening, so as to form chambers through which the gases to be employed (generally hydrogen and air) are passed, so that one side of the diaphragm is exposed to the one gas and the other to the other gas, and the spaces between the diaphragms are so connected that these gases pass in contact with a number of diaphragms.

Of the numerous ways in which dry gas batteries can be constructed we will describe two. One of these constructions, suitable for laboratory work, shown in fig. 1, consists of an earthenware plate M, impregnated with sulphuric acid and cemented into an ebonite frame R.

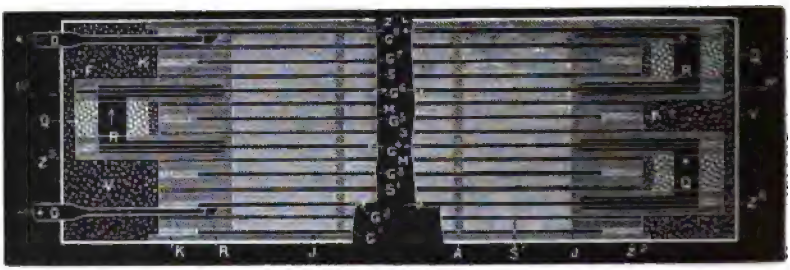
At a short distance from and all around the plate a copper wire A is let into the ebonite frame. The earthenware plate is covered with platinum leaf which has been perforated with a very large number (1500 per square cm.) of small holes, and which extends over and is in metallic contact with the copper wire. To protect the latter from corrosion, and to avoid local action, molten paraffin is put over the platinum leaf where it is in contact with the copper wire. Where the platinum leaf is in contact with the earthenware plate it is coated by

FIG. 1.



means of a brush with a very thin film of platinum black, which penetrates through the pores and holes of the platinum leaf, and thus comes into contact with the electrolyte. The frame R is fixed by means of screws between the two ebonite plates E, E′, with two india-rubber frames K, K′ intervening, thus forming two gastight chambers G, G′, through which the gases to be used are let by the tubes O, O′ and H, H′.

FIG. 2.



The second construction consists of a number of elements each of which is composed of two frames of lead and antimony consisting of a broad edge R, conducting strips A, and flaps F with holes and channels O, H, which form the inlets and outlets for the gases. These frames are coated with an insulating layer *a* of a mixture of gutta-percha, beeswax, resin, and paraffin. Between the two frames we insert a thin sheet S, prepared by coating a piece of cloth with plaster of Paris, and made impervious round the edges by the same insulating mixture, and then the open spaces formed by the conducting strips A are filled up with plaster of Paris mixed up with dilute sulphuric acid, so as to obtain an even plate. This is now coated with platinum leaf and platinum black in the same way as before described. A number of these elements are put side by side or one above the other, with non-conducting frames K of pasteboard intervening, so as to

FIG. 3.

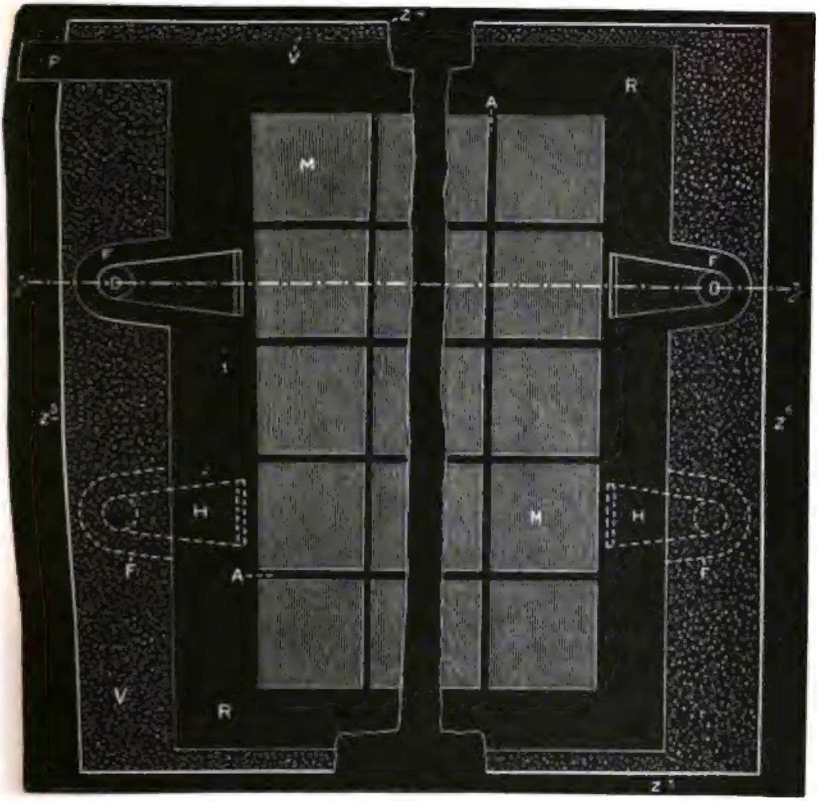
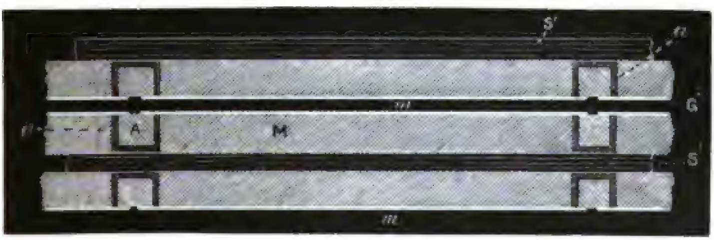


FIG. 4.



form gas chambers G_1, G_2, G_3, which are connected by means of cork or rubber washers Q, so that the gases admitted by the pipes O pass through the whole series of chambers. The first and the last gas chamber are formed by an additional pasteboard frame intervening between the last plates and two plates of zinc Z_1, Z_2, extending beyond the battery plates, which hold the set together. The whole

set of plates is now coated on the four sides not covered by the zinc plates with a mixture of beeswax and resin, so as to obtain a block which is perfectly gastight all round, and the space V left between the two zinc plates is then filled up with plaster of Paris, so as to obtain one solid compact block with no openings except the entrances and exits for the gases. This is closed by four more zinc plates, Z_3, Z_4, Z_5, Z_6, which are soldered together so as to form a box.

The E.M.F. of this battery we found to vary considerably, according to the way in which the platinum black is prepared. The best and most regular results we have obtained from platinum black made by neutralising a boiling solution of $PtCl_4$ with Na_2CO_3, and adding this slowly to a boiling solution of sodium formiate. With this we obtain an electromotive force of 0·97 volt with the open circuit. The internal resistance varies considerably with the thickness of the porous plates, the amount of the electrolyte contained in these, and the surface of these plates. Plates of gypsum of 8 mm. thickness and 350 cm. surface gave an internal resistance of 0·02 ohm.

The current obtainable from these batteries varies necessarily with the external resistance. It is possible to obtain 8 ampères from one such element, but the E.M.F. of the battery sinks at a very much more rapid rate than with constant batteries if a strong current is taken out, and the work done by the battery is not, as in constant batteries, at its maximum when the internal and external resistance are equal. Thus we found with a small battery of 42 sq. cm. surface and 0·36 ohm internal resistance the following results :—

R.	R_1.	P.	S.	E.	E_1.	D.	A.
0·360	0·30	0·265	0·883	0·62	0·58	0·04	0·236
„	0·40	0·310	0·775	0·62	0·58	0·04	0·240
„	0·70	0·430	0·614	0·67	0·65	0·02	0·263
„	1·00	0·490	0·490	0·68	0·66	0·02	0·240
„	2·00	0·605	0·302	0·72	0·71	0·01	0·219
„	4·00	0·700	0·175	0·77	0·76	0·01	0·122
„	8·00	0·780	0·097	0·81	0·81	0·00	0·075
„	20·00	0·860	0·043	0·87	0·87	0·00	0·036

R. Internal resistance in ohms.
R_1. External resistance in ohms.
P. Difference of potential at the poles.
S. Strength of current in ampères.
E. E.M.F. in volts, determined by the first deflection of the galvanometer needle 1 second after opening the circuit.
E_1. Calculated E.M.F. in volts.
D. Difference of E and E_1 = increase of E.M.F. during 1 second, showing rapidity of absorption of the gases with varying saturation of the platinum.
A. Work done by the battery in watts.

These figures show that in the case of the battery experimented with the maximum of work was obtained with an external resistance of about double the internal resistance.

This result is probably due, as pointed out by Dr. C. A. Wright,* to the fact established by Favre† and Berthelot,‡ that the gases occluded or condensed by platinum black evolve less and less heat per unit weight of gas the more gas the platinum black had previously condensed.

The heat evolved by the condensation of the gases by the platinum black, or a certain portion of this heat, is in all probability lost for the production of the current; it follows that the more the platinum black is saturated the less energy will be lost by the condensation of the gases, and *vice versâ*. Now, probably the rate of absorption of gas by the platinum black will rapidly diminish as it is more and more saturated with gas, so that in order to maintain it saturated or nearly saturated only a moderate amount of current can be obtained from a given surface, while if it is kept far below the saturating point it will condense the gases very rapidly, and a very large current can consequently be obtained.

As a practical limit we prefer to work the battery with an E.M.F. with closed circuit of about 0·73 volt. This allows us to take from 2 to $2\frac{1}{2}$ ampères (1·45—1·82 watt) out of an element with an active surface of 700 sq. cm., covered with 0·35 gr. of Pt leaf and 1 gr. of Pt black, which gives a useful effect of very nearly 50 per cent. of the total energy contained in the hydrogen absorbed in the battery.

We have found practically no difference in these results, whether we were using O and H or air and gases containing 30 per cent. to 40 per cent. of H, such as can be obtained by the action of steam or air and steam on anthracite, coke, or coal.

With a useful effect of 50 per cent., one-half of the heat produced by the combination of the H with the O is set free in the battery, and raises its temperature. By passing through the battery a sufficient excess of air, we can keep the temperature of the battery constant at about 40° C., and at the same time carry off the whole of the water formed in the battery by means of the gases issuing from it, so that the platinum black is kept sufficiently dry, and the porous plate in nearly the same state of humidity.

The E.M.F. of the open battery is very considerably below what it should be according to Thomson's theorem. The combustion of H and O should produce an E.M.F. of 1·47 volts, while we only obtain 0·97. It does not seem to us probable that this difference can be explained in the same way as the deviations from this theorem in a number of

* 'Phil. Mag.,' vol. 9, 1881, p. 169.

† 'Comptes Rendus,' vol. 77, 1873, p. 649.

‡ 'Annales de Chimie,' vol. 30, 1883, p. 519.

abnormal voltaic batteries have lately been explained by Chroustchoff and Sitnikoff,* viz., by the Peltier effect, which would probably not be different for the combinations HPt,SO_4H_2 and SO_4H_2,PtO; nor do the causes by which Herroun† explains this deviation appear to us to be applicable to the gas battery. It seems more probable—and what we have stated above respecting the rapid loss of the E.M.F. when taking out larger currents favours this view—that this loss of energy is to some extent due to the heat given out in the condensation of the gases by the platinum black.

Favre‡ found the heat given out by the condensation of 1 gram of H by platinum to vary from 23,000 to 13,000 calories, and concluded that this condensation was analogous to the condensation of carbonic acid by carbon, a purely capillary action. He did not determine the heat of condensation of oxygen.

Berthelot§ found the heat of condensation of H by platinum to vary per gram of H condensed from 17,000 to 8700 cal., and concluded that the H formed two distinct combinations with the platinum, the first taking place with a disengagement of 17,000 cal., and then combining with another equivalent of H with a disengagement of 8700 cal.

Berthelot also attempted to determine the heat given out by the absorption of O by platinum, which gas he found to be absorbed only in very small quantities, so that he could not determine the caloric effect with any amount of certainty; but he calculates it from the figures he obtained at at least 17,000 cal. for 8 grams of O. But these figures would lead to a much larger loss of energy than we find actually to take place. According to Berthelot, the condensation by platinum of 1 gram of H and 8 grams of O produces 25,700 to 34,000 cal. We obtain in the battery out of 34,187 cal. (resulting from the combination of 1 gram of H with 8 grams of O) 23,512 cal. as electricity, thus losing 11,666 cal. We are engaged upon an investigation of this rather difficult subject, with a view to further elucidating its effect upon the gas battery.

The fact that PdH, which, according to Favre,|| is formed with an evolution of only 4150 cal. per 1 gram of H (a figure which agrees fairly well with that obtained by calculation from the tension of PdH), produces, if opposed to PtO, a smaller E.M.F. than PtH, has also to be considered in studying this question.

Using Pd black on gold foil opposed to Pt black on Pt foil in our battery, we found the E.M.F. $PdH,H_2SO_4,PtO = 0{\cdot}91$ volt, as compared to $0{\cdot}97$ volt for PtH,H_2SO_4,PtO.

* 'Comptes Rendus,' vol. 108, 1889, p. 987.

† 'Phil. Mag.,' vol. 27, 1889, p. 209.

‡ 'Comptes Rendus,' vol. 77, 1873, p. 649.

§ 'Annales de Chimie,' vol. 30, 1883, p. 519.

|| 'Comptes Rendus,' vol. 68, 1869, p. 1525.

In the hope of throwing some light upon the question of the disappearance of energy in the gas battery, we have determined the E.M.F. of the following combinations by means of a block of plaster of Paris impregnated with sulphuric acid, one end of which was covered with platinum foil and platinum black and arranged so that it could be exposed to H or O, while the other end was plunged into a beaker which contained the liquid electrolyte and the electrode which we wished to examine.

	Found.		Theory.	Difference.
$PtH,H_2SO_4.PtO$	= 0·97 volt	= 22,512 cal.*	instead of 34,178 cal.†	−11,666.
Zn,H_2SO_4,PtO	= 1·77 volt‡	= 41,078 cal.	instead of 53,043 cal.	−11,965.
Cd,H_2SO_4,PtO	= 1·425 volt‡	= 33,171 cal.	instead of 44,928 cal.	−11,767.
Cu,H_2SO_4,PtO	= 0·70 volt‡	= 16,245 cal.	instead of 27,978 cal.	−11,733.
$PtH,H_2SO_4,CuSO_4,Cu$	= 0·31 volt	= 7,194 cal.	instead of 6,200 cal.	+994.
$PtH,H_2SO_4HNO_3,C$	= 1·19 volt	= 27,617 cal.	instead of 29,175 cal.	−1,558.
PtO,H_2SO_4,HNO_3,C	= 0·22 volt.			

These figures show that the loss of energy is very nearly the same per equivalent of O consumed, when PtO is used as the negative electrode, whether PtH, zinc, cadmium, or copper is used as positive electrode, and also that PtH with copper in copper sulphate, or carbon in nitric acid, as negative electrodes, gives nearly the theoretical E.M.F. It would thus seem as if the loss of energy in the gas battery occurred on the PtO electrode; but the question is undoubtedly a complicated one, and requires further study before an explanation of it can be attempted.

This battery differs from all other gas batteries in showing all the characteristics of polarisation after it has been at work for some length of time. It loses within an hour from 4 to 10 per cent. of its E.M.F. As the chemical processes taking place at the electrodes could not explain this, we had to look out for its cause in another direction, and found it to be the transport of the sulphuric acid from the O to the H electrode, resulting in the acid becoming gradually more concentrated on the positive side and weaker on the other, which we have established by analysing the gypsum scraped off below the platinum leaf at both sides. Probably this difference of concentration of the acid sets up a counter-current. In order to counteract this disturbing influence and to keep the current constant, we interchange the gases in the battery from time to time, say once an hour, so that the current goes in an opposite way through the porous diaphragm, and transports the sulphuric acid back. This necessitates,

* 1 Daniell = 25,065 cal. = 1·08 volts.

† Calculated from Thomsen's data ('Thermochemische Untersuchungen') divided by 2, so as to refer to O = 8.

‡ Wright and Thompson found 1·75, 1·5, and 0·78 respectively ('Roy. Soc. Proc.,' vol. 44, 1888, p. 182).

where constant currents are wanted for a longer period, the working of a number of elements or batteries connected by means of a commutator in such a way that one element or battery will always be out of the circuit, and have its gases changed, and be replaced in the circuit at the moment when the next element or battery is switched out for the same purpose.

In using, in place of sulphuric acid, a solution of sodium chloride as electrolyte, we found, after working the battery for some time, sodium hydrate on one side and HCl on the other side of the battery, and have been able to determine in this case the polarisation to be equal to 0·54 volt, which very nearly accounts for the difference between the E.M.F. of the open battery and the E.M.F. calculated according to Thomson's theorem. The E.M.F. of PtH,NaClAq,PtO we found equal 0·86 volt, which, added to the polarisation of 0·54 volt just mentioned, gives a total of 1·40 against the theoretical figure for H, O = 1·47. By changing the gases after the polarisation was fully established, the battery showed an E.M.F. of 1·39 volts.

This observation, as well as the determinations of Peirce,* of the E.M.F. of gas batteries with the same gases and different electrolytes, shows that the electrolyte also has considerable influence upon the E.M.F.

We hope by further investigation to arrive at assigning their proper value to the various causes affecting the E.M.F. of gas batteries.

VI. "Contributions to the Chemistry of Storage Batteries. No. 2." By E. FRANKLAND, D.C.L., F.R.S. Received June 18, 1889.

Under this title I communicated to the Royal Society, in February, 1883,† the results of some experiments on the reactions occurring during the charging and discharging of a storage cell. I showed that no appreciable part of the storage effect was due to occluded gases, as had been previously suggested by some chemists and physicists; but that the act of charging consisted essentially in the decomposition of lead sulphate whilst the discharge was produced by the recomposition of this salt.

The establishment of these, as practically the only reactions going on in a storage cell, enabled me to prescribe a very simple method by which the charge in any cell could be ascertained; for as sulphuric acid is liberated during the charging and absorbed by the active material of the plates during discharge, the amount of charge could

* 'Wiedemann, Annalen,' vol. 8, 1879, p. 98.

† 'Proceedings of the Royal Society,' vol. 35, p. 67.

at any time be measured by ascertaining the amount of free sulphuric acid in the cell; in other words, by simply determining the specific gravity of the electrolyte; and this method has since been very generally adopted by the users of storage batteries.

In continuing these experiments, it soon became evident that the lead sulphate formed and decomposed in the cell could not be the ordinary white sulphate hitherto known to chemists, because, in the first place, the active material of the plates always remains coloured even after discharge, and secondly, because whenever white sulphate is produced through abnormal reactions in the cell, it is afterwards decomposed only with extreme difficulty by the electric current.

In order to obtain some light upon the composition of the sulphate formed and decomposed in the cell, I have studied the action of dilute sulphuric acid upon litharge and minium, the two oxides of lead chiefly used in the construction of the plates of storage cells.

Action of Dilute Sulphuric Acid on Litharge.

Finely powdered litharge was treated with successive portions of dilute sulphuric acid until the liquid remained strongly acid after prolonged trituration. The resulting insoluble buff-coloured powder was washed with water till free from acid, and dried, first at 100° C. and afterwards at 150—160°. The loss at this higher temperature was less than 0·2 per cent., and was therefore due to hygroscopic moisture.

PbO and SO_3 were then determined in the dried compound as follows:—The salt was dissolved in a small quantity of pure concentrated solution of caustic potash, and the solution, after dilution, was saturated with CO_2. (According to H. Rose, $COPbo''$ is soluble in $COKo_2$, but not in $COHoKo$.) Any excess of CO_2, which might have caused the $COPbo''$ to dissolve, was avoided by warming the liquid with the precipitate on the water-bath to a temperature at which the $COHoKo$ begins to dissociate. The liquid was then allowed to cool and to stand twelve hours before filtering. The $COPbo''$ was filtered off, converted into nitrate, and precipitated and weighed as sulphate. The sulphuric acid was determined in the filtrate from the $COPbo''$.

1·2964 grams of the salt gave 0·6647 gram baric sulphate and 1·4437 gram plumbic sulphate.

These numbers agree closely with the formula—

$$(SO_3)_3(PbO)_5,$$

as is seen from the following comparison of calculated and experimental numbers:—

	Calculated.			Found.
$3SO_3$	240	17·71	....	17·61
$5PbO$	1115	82·29	....	81·96
	1355	100·00		99·57

These analytical results suggest the following graphic formula :—

$$\begin{array}{ccccc} PbO_2 & & & & PbO_2 \\ \| & & & & \| \\ S & =O_2= & S & =O_2= & S \\ \| & & \| & & \| \\ PbO_2 & & PbO_2 & & PbO_2 \end{array}.$$

The formation of this salt may be represented by the following equation :—

$$\underset{\text{Litharge.}}{5PbO} + \underset{\text{Sulphuric acid.}}{3SO_4H_2} = \underset{\text{Buff lead salt.}}{S_3Pb_5O_{14}} + \underset{\text{Water.}}{3OH_2}.$$

Action of Dilute Sulphuric Acid on Minium.

Minium was treated with dilute sulphuric acid in exactly the same way as litharge, and the resulting brownish red compound dried, first at 100° C., and afterwards at 150—160°. The loss at this higher temperature was again less than 0·2 per cent.

PbO, SO_3, and excess of oxygen were then determined in this salt in the following manner :—The salt was first treated with concentrated hydrochloric acid in order to reduce all the lead to the monoxide stage. The resulting mixture was then dissolved in caustic potash and treated as already described. The excess of oxygen was determined by finding the loss of weight which resulted from the evolution of CO_2 when the salt was treated with oxalic acid and dilute nitric acid.

2·1136 grams of the salt gave 1·1978 grams baric sulphate and 2·2710 grams lead sulphate.

1·5110 gram treated with oxalic acid and dilute nitric acid evolved 0·0910 gram CO_2.

These numbers correspond to the following percentages :—

SO_3	19·46
PbO..................	79·08
O..................	1·09
	99·63

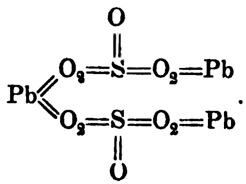
which agree with the formula—

$$S_2Pb_3O_{10},$$

as is seen from the following comparison:—

	Calculated.			Found.
S_2	64	7·57	....	7·78
Pb_3	621	73·49	....	73·41
O_{10}	160	18·94	....	18·44
	845	100·00		99·63

The composition of this salt may be represented graphically thus:—



The formation of this salt is expressed by the following equation:—

$$\underset{\text{Minium.}}{Pb_3O_4} + \underset{\text{Sulphuric acid.}}{2SO_4H_2} = \underset{\text{Red lead salt.}}{S_2Pb_3O_{10}} + \underset{\text{Water.}}{2OH_2}.$$

These then are the salts which constitute the original active material of storage cells when that material is formed by the admixture of sulphuric acid with litharge or minium respectively, and it is highly probable that one or the other of these salts takes part in the electrolytic processes of the storage battery. It is fortunate that these hitherto unknown salts (and not the ordinary known sulphate) are formed in the cell reactions; for, in the alternative case, lead storage batteries would be practically valueless.

If the buff lead salt be the active material of the battery plates, then the following equations express the electrolytic reactions taking place in the cell:—

I. In charging—

(*a.*) *Positive Plates.*

$$\underset{\text{Buff lead salt.}}{S_3Pb_5O_{14}} + \underset{\text{Water.}}{3OH_2} + O_5 = \underset{\text{Lead peroxide.*}}{5PbO_2} + \underset{\text{Sulphuric acid.}}{3SO_4H_2}.$$

(*b.*) *Negative Ptates.*

$$S_3Pb_5O_{14} + 5H_2 = 5Pb + 3SO_4H_2 + 2OH_2.$$

* Mr. Fitzgerald considers that this peroxide is hydrated.

II. In discharging—

(a.) Positive Plates.

$$5PbO_2+3SO_4H_2+5H_2 = S_3Pb_5O_{14}+8OH_2.$$

(b.) Negative Plates.

$$5Pb+3SO_4H_2+O_5 = S_3Pb_5O_{14}+3OH_2.$$

If the red lead salt be the active material, then the following equations express the same electrolytic reactions:—

I. In charging—

(a.) Positive Plates.

$$\underset{\text{Red lead salt.}}{S_2Pb_3O_{10}}+O_2+2OH_2 = \underset{\text{Lead peroxide.}}{3PbO_2}+\underset{\text{Sulphuric acid.}}{2SO_4H_2}.$$

(b.) Negative Plates.

$$S_2Pb_3O_{10}+4H_2 = 3Pb+2SO_4H_2+2OH_2.$$

II. In discharging—

(a.) Positive Plates.

$$3PbO_2+2SO_4H_2+2H_2 = S_2Pb_3O_{10}+4OH_2.$$

(b.) Negative Plates.

$$3Pb+2SO_4H_2+2O_2 = S_2Pb_3O_{10}+2OH_2.$$

An inspection of these equations discloses, in the case of the red lead salt, a fact which has already been roughly observed in practice, viz., that only half as much active material is electrolytically decomposed on the negative as on the positive plates; whence it follows that the weight of active material on the negative plates need not exceed one-half of that upon the positive plates; for, in the decomposition of the electrolyte, equivalent quantities of oxygen and hydrogen are evolved; that is to say, two atoms of hydrogen for each atom of oxygen. But, in the decomposition of the red lead salt, four times as many atoms of hydrogen are required to reduce the salt to metallic lead as atoms of oxygen which are necessary to transform the lead of the salt into peroxide. When, however, the active material of the positive plate has once been converted into peroxide of lead, it seems probable that the red salt only is formed; at all events until the discharge at high potential is nearly completed, when there are indications of the production of the buff-coloured salt. But this is a point requiring further investigation.

I have to thank Dr. F. R. Japp, F.R.S., for his assistance in the analytical work of this investigation.

VII. "Contributions to the Anatomy of Fishes. I. The Air-bladder and Weberian Ossicles in the Siluridæ." By T. W. BRIDGE, M.A., Professor of Zoology in The Mason College, Birmingham, and A. C. HADDON, M.A., Professor of Zoology in the Royal College of Science, Dublin. Communicated by Professor A. NEWTON, F.R.S. Received June 12, 1889.

Weber, in his classical memoir entitled 'De Aure et Auditu Hominis et Animalium. Pars. I.—De Aure Animalium aquatilium,' published in 1820, was the first to show that in certain families of Physostomous Teleostei, which were subsequently grouped together under the name of Ostariophyseæ by the late Dr. Sagemehl, there exists a peculiar connexion between the membranous labyrinth of the internal ear and the air-bladder by means of a chain of movably interconnected "auditory" ossicles. Of the four families (Gymnotidæ, Characinidæ, Gymnarchidæ, and Siluridæ) in which this singular mechanism is present, the Siluridæ have received but comparatively scanty attention since the publication of Weber's paper. Weber himself only described the air-bladder and auditory ossicles of one species (*Silurus glanis*). Johannes Müller, in his various communications to the Berlin Academy during the years 1843–45, added somewhat to our knowledge of these structures, and notably by his discovery of the *springfederapparat*; but Müller's attention was mainly directed to the grosser features in the anatomy of the air-bladder to the entire exclusion of all but the slightest reference to the important skeletal modifications which are associated with the peculiar structure of that organ in the Siluridæ. Reissner has given a fairly complete account of the bone-encapsuled air-bladder of *Rhinelepis*, but by far the most valuable of the more recent contributions to this branch of vertebrate morphology are the papers by Professor Ramsay Wright, relating to the aberrant Siluroid *Hypothalmus*, and to the more normal North American species, *Amiurus catus*. In his papers on *Amiurus*, Professor Wright was not only the first to describe the fusion of the second, third, and fourth vertebræ in the formation of what we have termed the "complex vertebra"—a fact which could hardly have been discovered except through embryological evidence—but was also the first to give an accurate account of the skeletal relations and attachments of the air-bladder in any one Siluroid.

In this preliminary communication we propose to state briefly the results of our investigations into the various modifications which the air-bladder and the "auditory ossicles" undergo in ninety-two

species of Siluridæ, referable to about fifty genera, and mainly belonging to Dr. Günther's sub-families of Siluridæ Homalopteræ, S. Heteropteræ, S. Proteropteræ, and S. Proteropodes. For the present we shall only give a brief *résumé* of the morphological variations in the structures concerned, leaving their physiological bearing and the more general conclusions which the facts elucidated suggest, to a future and more detailed communication to this Society; but before doing so we venture to suggest the need of a revision of the customary nomenclature of the so-called "auditory ossicles." From a mistaken idea of their homology with the Mammalian auditory ossicles, Weber gave to three of them the suggestive but extremely misleading names of "incus," "malleus," and "stapes." Since Weber's time, however, it has become obvious that the "auditory" ossicles of the Ostariophyseæ are in no sense homologous with the similarly named bones of the Mammalian tympanum, and it is almost equally clear that the two series of ossicles have nothing in common with regard to their respective functions. With a view of avoiding all possibility of the confusion which may result from applying identical terms to structures widely different in origin and probably equally remote in function, we venture to suggest a different nomenclature for the "auditory ossicles" of Weber. Instead of stapes we propose the name "scaphium," in allusion to the invariably concavo-convex or spoon-shaped form of this ossicle. The "incus" may be renamed the "intercalarium," from its constant intermediate position between the "stapes" and the "malleus," when present. For "malleus" we would substitute "tripus"—a name suggested by the three characteristic processes which this ossicle invariably possesses. The fourth ossicle, called the "claustrum" by Weber, forms one of the series of auditory ossicles in the Cyprinoid fishes, but has no such physiological significance in the Siluridæ, although it is very generally present. As the name "claustrum" is open to none of the objections which can reasonably be urged against the retention of Weber's nomenclature of the three preceding ossicles, it may with advantage be retained. For the ossicles collectively, including the claustrum, and for obvious reasons, we would suggest the name "Weberian ossicles" as an appropriate designation, instead of "auditory ossicles."

In summarising the more noteworthy of the results of our investigations into the morphology of the air-bladder and Weberian ossicles, and the correlated modifications which the anterior vertebræ and their processes undergo, we may indicate, in the first instance, such features as appear to be common to nearly all Siluroids, and secondly, those that are characteristic of particular genera or species.

Although only demonstrated in one particular instance (the young of *Amiurus catus*) by Ramsay Wright, our researches lead us to

believe that the great majority of Siluroids agree with *Amiurus* in having the centrum of the second vertebra, and the centra, neural arches, and spinous processes of the third and fourth vertebræ indistinguishably combined to form an apparently single vertebra, for which we venture to suggest the name of "complex vertebra." The discovery by Baudelot that the "complex vertebra" of the Cyprinidæ was formed by the fusion of the second vertebral centrum with the third vertebra, was due to the distinctness of these elements in one particular species, but no evidence of a similar nature is available in any but embryonic Siluridæ. In no adult Siluroid is there the slightest trace of intervertebral spaces or sutures between the three confluent centra, in fact, the only features which in any way suggest the composite nature of the complex vertebra in that family are the perforation of its neural arch by two pairs of spinal nerves and the occasional presence of two pairs of nutrient foramina on the ventral surface of its centrum. This fusion of vertebræ in the formation of the "complex" is almost invariably attended by the partial anchylosis of the latter to the fifth vertebra, partly as the result of the firm sutural union of their correlated elements, and in part due to the investment of the lateral surfaces of their centra by a continuous deposit of superficial bone. Moreover, the conjoined vertebræ, with the addition of the centrum of the first, are so articulated to the skull that little, if any, motion is possible, either between the individual vertebræ or between the latter and the skull. The centrum of the first vertebra is nearly always more or less rudimentary.

With the possible exception of the claustra no distinct or ossified intercalary elements are ever present.

The first vertebra very rarely has transverse processes, and even when present (*e.g.*, some species of *Arius*) they are extremely rudimentary. The transverse processes of the fourth vertebra, on the contrary, are always greatly expanded, not infrequently divided into anterior and posterior division by a cleft, and with or without the aid of those belonging to the fifth vertebra form a more or less complete investment to the dorsal and anterior walls of the air-bladder. The sixth is, as a rule, the first rib-bearing vertebra; exceptionally, however, it may be the fifth (*Callichrous*), or even the seventh (*Clarias*).

In almost all cases, except where they are modified to form an "elastic-spring-apparatus," the transverse processes of the fourth vertebra, in addition to their characteristic relations to the air-bladder, form a more or less rigid support to the proximal elements of the pectoral girdle.

Over a somewhat triangular area, on each side, between the exoccipital in front and the anterior margin of the arch of the complex vertebra behind, the wall of the neural canal is formed only by fibrous

membrane, in which the claustrum and the ascending process of the scaphium are imbedded.

Of the four Weberian ossicles the claustrum has no physiological relations to the atrial cavities (*atria sinus imparis* of Weber), but merely strengthens the wall of the neural canal behind the exoccipital. Each scaphium has a spatulate process which fits into and completely closes the corresponding external atrial aperture, and at the same time forms the outer wall of the atrial cavity of its side, and also a rounded condylar process for articulation with the centrum of the first vertebra. The intercalarium is usually represented by an elongated or discoidal nodule imbedded in the stout ligament ("interossicular ligament") connecting the scaphium with the tripus, and even if horizontal and ascending processes are present, the ossicle never articulates with the centrum of the second vertebra to which, as a modified neural arch, it belongs. The tripus is always a tripartite ossicle with its posterior or crescentic process imbedded in the dorsal wall of the air-bladder; the anterior process is directed forwards parallel to the long axes of the complex and first centra, and opposite the external atrial aperture of its side is connected by the transversely-disposed interossicular ligament with the convex outer surface of the spatulate process of the scaphium. The articular process usually articulates with the lateral surface of the vertebral centrum (the third), of which it is a modified transverse process; very rarely (*e.g.*, *Auchenipterus*) is the process directly continuous with the neural arch.

The Weberian ossicles, or at all events the free portion of the tripus and the intercalarium, are enclosed within a membranous saccus paravertebralis, the anterior wall of which is perforated by the interossicular ligament as the latter passes forwards from the tripus to its attachment to the scaphium. Unlike the Cyprinidæ, the complete closure of the external atrial aperture by the spatulate process of the scaphium and the minute size of the hypoglossal foramen in the Siluridæ completely cut off all communication between the cavity of the saccus and the cranial cavity.

The first spinal or hypoglossal nerve perforates the exoccipital. The second and third spinal nerves emerge from the neural canal between the claustrum anteriorly and the arch of the complex vertebra behind, but are invariably separated by the ascending process of the intercalarium whenever that process is developed, as in *Macrones*, *Liocassis*, and *Bagroides*. The fourth and fifth spinal nerves traverse the neural arch of the complex vertebra, and the sixth, the arch of the fifth vertebra. The additional spinal nerve described by Sagemehl in *Silurus glanis* as emerging between the claustrum and the ascending process of the scaphium, we have never met with, although our attention has been specially directed to that point.

The air-bladder varies greatly in degree of development, not only in different genera, but in different species of the same genus. Even individual variations are not infrequent. Very rarely does it exhibit the bipartite division into an anterior and a posterior sac so characteristic of other families of Ostariophyseæ. One of its most noteworthy features is a tendency to lateral development, whereby the outer walls of the anterior portion become applied, through the divergence of the dorso-lateral and ventro-lateral muscles of the body wall, directly to the external skin ("lateral cutaneous areas"). The insertion of the crescentic processes of the tripodes is always into the dorsal wall of the anterior chamber of the air-bladder in the normal Siluridæ, or into the corresponding walls of the laterally situated air-sacs in the abnormal forms, and takes place in such a way that the fibres forming the anterior and lateral walls of each half of an anterior chamber, or of each air-sac, converge as they pass into and form the dorsal wall, and ultimately become inserted into the convex outer margin of the tripus of that side. Specialised fibres of the dorsal wall ("radial fibres") converge like the radii of a circle from the inner concave margin of the crescentic process, and are inserted either directly into the adjacent lateral surface of the complex centrum or indirectly through the intervention of an osseous nodule ("radial nodule"). Except, perhaps, in cases where the same effect is produced by their partial encapsulation by bone, the anterior chamber of the bladder, and its equivalent the laterally-situated air-sacs of the abnormal Siluridæ, generally have their walls so attached to, or buttressed by, rigid portions of the axial skeleton, that only their outer or lateral walls are capable by inward or outward bulging of allowing variations in the internal capacity of the bladder to take place.

A ductus pneumaticus is very generally, but not invariably, present.

In nearly all Siluroids the lateral growth of the air-bladder, and the intimate relation of its outer walls to lateral cutaneous areas, have led to the displacement of the lateral lobes of the liver and their enclosure within peritoneal *cul-de-sacs*—a condition which usually persists even in cases where the air-bladder has undergone partial atrophy.

In many of the features to which reference has just been made, the Siluridæ differ from all the other families in which a Weberian mechanism is present.

As a convenient means of summarising the more important generic and specific variations, the Siluroids may be somewhat arbitrarily divided into two principal groups:—(I) the *Siluridæ normales*, and (II) the *Siluridæ abnormales*. In the former group the air-bladder is always well developed and subdivided internally into three inter-

communicating compartments, of which one is anterior and two posterior or lateral in position. The anterior and dorsal walls of the anterior chamber may be more or less completely invested by the modified transverse processes of the fourth and fifth vertebræ, but the latter do not form deep concave recesses or capsules for the partial or complete enclosure of the entire air-bladder.

In the Siluridæ abnormales on the contrary, the air-bladder is always small relatively to the size of the Fish, and more or less degenerate, sometimes partially solid, but almost invariably includes two laterally situated air-sacs with simple cavities, which together may be regarded as equivalent to the anterior chamber of a normal Siluroid. Lateral compartments, as a rule, are absent altogether, or, if present, are very rudimentary. Whatever its condition, the air-bladder is almost always partially or completely enclosed within transversely disposed bony recesses, formed either by the transverse processes of the fourth vertebra alone, or in conjunction with those of the fifth vertebra.

Although a convenient method of classifying morphological facts, it is obvious that this classification, based as it is upon so variable an organ as the air-bladder, can have no genetic value.

(I.) *Siluridæ Normales.*

Under this head may be included such genera as:—*Plotosus*, *Wallago*, *Callichrous*, *Cryptopterus* (certain species). *Eutropius*, *Pangasius*, *Macrones*, *Rita*, *Pimelodus* (certain species), *Piramutana*, *Arius*, *Osteogeniosus*, *Oxydoras*, *Malapterurus*, and others.

In the Siluroids included in this group the number of rigidly interconnected vertebræ varies. The first, the complex, and the fifth vertebræ are almost invariably so connected together that no motion is possible between them, and occasionally the sixth, the seventh, and even the eighth may be included in the series. The rigidity of the complex and fifth vertebræ, with the occasional addition of the sixth, may be further increased by the sutural union or partial anchylosis of their respective transverse processes. The anterior vertebræ are also firmly connected to the skull, generally by the articulation of the arch and spine of the third vertebra with the exoccipitals and supraoccipital, the transverse processes of the fourth vertebra with the post-temporals, and the spinous processes of the third and fourth vertebræ with the supraoccipital spine; less frequently by the formation of interlocking accessory articular processes on the contiguous ventral margins of the basioccipital and the centra of the first and complex vertebræ (*Macrones*, *Bagrus*). In *Arius* the accessory processes are very strongly developed, and by their downward growth and coalescence, form a stout, conical subvertebral process for the

attachment and support of the anterior wall of the air-bladder. In some Siluroids the connexion of the skull with the anterior vertebræ may be rendered still more intimate by the articulation of the supra-occipital spine with the expanded dermal plates of the first and second interspinous bones, as in *Auchenipterus*, *Oxydoras*, &c., or even by the downward growth of paired processes from the supraoccipital to unite with the dorsal surfaces of the transverse processes of the fourth vertebra, as in *Arius*, *Batrachocephalus*, &c.

The centrum of the first vertebra varies greatly in size, but is always the smallest of the anterior vertebræ. Two pit-like sockets are always found on its dorsal surface for the reception of the globular condylar processes of the scaphia. The complex and fifth centra are the largest, or, at all events, the longest of the anterior vertebræ, and, as a rule, their anterior and posterior concavities are unsymmetrically developed. In nearly all cases these centra are not only elongated but laterally compressed, so as to form a prominent subvertebral keel, which gives rise to a deep groove along the medio-dorsal line of the anterior chamber of the air-bladder, and, at the same time, internally, to a prominent longitudinal ridge, partially subdividing the cavity of the chamber into two laterally bulging halves. A fan-shaped subvertebral process may be developed from the ventral and anterior margin of the complex centrum for the support of the anterior wall of the bladder (*e.g.*, *Auchenaspis*), and the lateral surface of the same centrum is not infrequently thickened into oblique lateral ridges for the dorsal attachment of the anterior pillars of the anterior chamber. For the same purpose a variously shaped osseous nodule ("radial nodule") is attached to the dorsal extremity of each ridge, or in its absence directly to the centrum, and is either confluent therewith, or suturally, or by fibrous tissue only, connected thereto. The radial nodules in addition to serving for the attachment of the "anterior pillars" receive also the insertion of the radial fibres of the tripus. Almost invariably a thin slender lamina of bone, the "radial ridge," is prolonged from each radial nodule, and, after passing obliquely upwards, outwards, and backwards, ventral to the posterior cardinal vein, blends with the ventral surface of the transverse process of the fourth vertebra.

The neural arch of the complex vertebra is partially or completely anchylosed to the arch of the fifth vertebra, which, in turn, may be similarly connected with the arch of the sixth vertebra, or the rigid union of the different neural arches may be effected by a firm sutural union.

The transverse processes of the fourth vertebra, very frequently those of the fifth vertebra, and more rarely those belonging to the sixth vertebra (*Platystoma*), are more or less expanded, and by their partial anchylosis or sutural union with one another, form on each

side of the vertebral column a broad, wing-like plate of bone, the anterior margin of which is strongly decurved, for the investment of the dorsal and anterior walls of the anterior chamber of the air-bladder. The transverse process of the fourth vertebra has a broad flat root, and may be simple, or, as is more usually the case, cleft more or less deeply into anterior and posterior divisions, of which the former is always decurved for a portion of its extent, and closely applied, even if not attached, to the lateral portion of the anterior wall of the bladder, in addition to its ligamentous or articular connexion with the post-temporal. In certain Siluroids the transverse process becomes modified to form the "elastic-spring-apparatus," first described by Johannes Müller. In some of these forms (*Malapterurus*, *Synodontis*, *Pangasius*) each anterior division is almost completely separated from the posterior division, with an oblique origin from the arch of the complex vertebra, and, becoming flexible and highly elastic, expands distally into a more or less oval plate, which is closely applied to the lateral portion of the anterior wall of the bladder. In others (*Auchenipterus*, *Oxydoras*) the anterior division also forms an "elastic-spring-apparatus," but the posterior division is entirely wanting. In all such cases the modified transverse processes are provided with powerful protractor muscles, which have their origin on the posterior face of the skull, and their insertion into the anterior surfaces of the oval plates.

The post-temporal bone always has a transversely, or obliquely, disposed inferior limb for articulation at its inner extremity with the lateral surface of the basioccipital, in addition to an ascending process for articulation with the pterotic and epiotic bones. Where the transverse process of the fourth vertebra fails to articulate with and support the post-temporal, as is the case in all Siluroids possessing an "elastic-spring-apparatus," the inferior limb of the latter is exceptionally massive, with an extensive articulation, or even partial anchylosis, with the basioccipital or, in addition, with the exoccipital also. In other genera (*Macrones*, *Bagrus*, &c.) the inferior limb, in conjunction with the body of the same bone, may form a bony expansion or post-temporal plate, which, with the produced crescentic distal extremity of the anterior division of the transverse process of the fourth vertebra, forms a slightly concave bony structure for the support of the lateral portion of the anterior wall of the bladder. From being but faintly concave on its posterior face, the post-temporal plate and the adjacent portion of the inferior limb may become deeply excavated to form a goblet-shaped cavity, into which a thin-walled cœcal diverticulum of the air-bladder extends (*Macrones aor*).

Apart from those which are characteristic of all Siluroids, no important modifications of the hinder part of the skull are observable in the normal members of the group, either as regards the more

general features of structure or the more special points involved in the mode of formation and relations of the "cavum sinus imparis," or of its bilobed backward prolongation, the "atria sinus imparis." The uniformity in the latter respect is so marked, that a description of those structures as they occur in any one normal Siluroid will practically apply to all the others.

As regards the internal ear, the condition of many of our specimens was such that our observations were necessarily somewhat incomplete. The condition of the membranous labyrinth, and its relations to the cavum sinus imparis and to the atrial cavities, were investigated in a large number of Siluridæ normales but with the purely negative result that we could detect no variations of any importance from the arrangement of these structures, already described for *Amiurus catus* by Ramsay Wright, and for *Silurus glanis* by Weber. In all cases we found a transversely disposed ductus endolymphaticus connecting the two sacculi, and, attached to the ductus, a median pear-shaped sinus endolymphaticus projecting backwards into, and almost completely filling, the "cavum sinus imparis."

With the exception of the intercalarium, the Weberian ossicles exhibit but little variety in shape or in their relations to one another or to the atrial cavities and air-bladder. The variations in the condition of the tripus relate principally to the degree and shape of the curvature of its posterior or crescentic process. In some genera (*Auchenipterus*, *Oxydoras*) the crescentic process is almost straight; in others almost hook-shaped (*Plotosus*); and between these extremes the process may exhibit almost every degree of curvature. A ventral ridge on the root of the crescentic process, to receive the insertion of a slip of fibres from the adjacent anterior wall of the bladder, is very generally present, and varies in size according to the thickness of the walls of the bladder. In some Siluroids (*Macrones*, *Liocasis*) the outer convex margin of the process may be increased for the purpose of fibrous attachment by the addition of an outwardly directed heel-like process. The articular process of the tripus is usually distinct from the complex centrum, with which, however, it articulates at the bottom of a deep pit-like depression. It is very rare, as in one genus (*Auchenipterus*), for the process to be flexible and elastic, and directly continuous by an oblique origin with the anterior part of the neural arch of the complex vertebra like the adjacent and similarly elastic root of the "elastic-spring-apparatus." The proportional lengths of the anterior and crescentic processes vary somewhat in different forms; generally, the two processes are of approximately equal length, but when otherwise it is the anterior which is the longer.

The intercalarium varies greatly in development. Usually a small osseous nodule imbedded in the interossicular ligament, the intercalarium may, in addition, be prolonged therefrom as a horizontal

spicule which terminates in the fibrous wall of the neural canal, between the arch of the complex vertebra and the ascending process of the scaphium, near the dorso-lateral margin of the anterior portion of the complex centrum, with which, however, it is in no way directly attached (*Cryptopterus*, *Callichrous*). In a few genera (*Macrones*, *Liocassis*, *Pseudobagrus*, &c.) the horizontal process is prolonged upwards into a vertically disposed or ascending process, which also lies in the fibrous wall of the neural canal, behind and parallel to the ascending process of the scaphium. In all cases where an ascending process is present it lies between the paired foramina for the exit of the dorsal and ventral roots of the second and third spinal nerves.

The only variations noticed in condition of the scaphium relate to the occasional absence of an ascending process.

Claustra are invariably present, but vary greatly in size, from the condition of extremely slender spicules to somewhat triangular plates (*Pangasius buchanani*).

The air-bladder has the same fundamental structure in all the S. normales. In all cases the organ is more or less cordate in shape, and is subdivided internally by a T-shaped arrangement of a primary transverse septum and a longitudinal septum into three intercommunicating compartments, of which one is anterior and transversely disposed, occupying the anterior third of the bladder, and two posterior or lateral longitudinally arranged chambers, constituting the posterior two-thirds of the bladder. The dorsal wall of the anterior chamber is closely moulded to the ventral and lateral surfaces of the complex and fifth centra, including the subvertebral keel which these centra form, and also to the ventral surfaces of the modified transverse processes of the fourth and fifth vertebræ. The lateral portions of the anterior wall of the chamber are also partially buttressed by the decurved anterior margins of the transverse processes of the fourth vertebra, with or without the aid of the expanded inferior processes of the post-temporals (post-temporal plates), while the median portion of the wall is not infrequently supported by a subvertebral process (*Arius*). The lateral compartments, on the other hand, are neither invested by bone, nor are they in any way directly attached to the skeleton, but lie free in the abdominal cavity. Except in relation to the size of the Fish, the variations in the capacity of the anterior chamber as compared with those of the lateral compartments are but slight, and, as a rule, any increase or diminution in the relative size of the bladder is mainly due to variations in the size of the lateral chambers. With the exception of two genera (*Rita* and *Aspredo*) included in this group, the capacity of the anterior chamber is always much smaller than the combined capacities of the two lateral chambers, and, in one of the two exceptions referred to, the partial suppression of the lateral compartments is compensated by the

development of two large lateral cæca from the anterior chamber. Apart from its partial longitudinal construction into two laterally bulging halves,—a separation which in some cases may be emphasised by the formation of one or two longitudinally arranged and inwardly projecting ridge-like aggregation of fibres from the median line of the posterior, ventral, and anterior walls,—the cavity of the anterior chamber has smooth walls, and is not subdivided by the growth of internal septa.

The lateral compartments may also have undivided cavities (*Auchenaspis*, *Callichrous*, and *Silurus*), but not infrequently they are rendered more inexpansible, and possibly at the same time less compressible, by the formation of a variable number of secondary transverse septa (*e.g.*, *Macrones*), which incompletely subdivide each chamber into a series of transversely arranged, intercommunicating spaces. Occasionally the excessive development of these septa and their union by root-like bundles of fibres, which pass between their opposed surfaces, may lead to the formation of a thick trabecular network of fibrous columns or bands, and to the partial obliteration of the cavities of the two chambers (*Pangasius*).

The width of the primary transverse septum forming the posterior wall of the anterior chamber varies greatly in different Siluroids. In some (*e.g.*, *Auchenaspis*) the septum is co-extensive with the width of the air-bladder, although contracted dorsally to admit of the lateral chambers communicating with the anterior chamber; in others (*Callichrous*, *Cryptopterus*) the septum is reduced to the condition of a narrow, but stout, column-like aggregation of fibres.

Cæcal appendages to the anterior and lateral compartments are not uncommon. The anterior chamber may have small anterior cæca (*Macrones aor*), or much smaller antero-lateral cæca (*Osteogeniosus*). Lateral cæca are sometimes present, and may either take the form of large funnel-shaped structures, which extend the whole length of the abdominal cavity and are entirely free from internal subdivisions (*Rita*), or may occur as small forwardly directed outgrowths, subdivided internally by a network of fibrous bundles, and communicating with the anterior chamber by a number of slit-like orifices in its lateral walls (*Platystoma*). Very rarely (*Callophysis*) are the lateral cæca so numerous as to form a wreath round the lateral regions of the chamber. The lateral compartments are frequently either constricted or prolonged into a posterior cæcal appendage. This may be a longer or shorter tubular, or a slightly oval structure, and confined to the abdominal cavity (*Pangasius buchanani*, *Bagroides melanopterus*), or a long, tapering, tubular structure, which, after traversing the abdomen, extends for some distance along the right side of the tail, between the hæmal arches and the lateral musculature (*Cryptopterus micronema*). In some cases the posterior cæcum is very large, and in

shape an elongated oval body (*Pangasius djambal*, *Malapterurus electricus*), or it may be flattened and leaf-like (*Pangasius macronema*). In one instance (*Oxydoras*) it is very rudimentary. The existence of a pair of rudimentary posterior cæca is very rare (*Auchenipterus obscurus*). Very generally the longitudinal septum extends backwards into the posterior cæcum, and subdivides its cavity into two distinct lateral canals or chambers, which communicate anteriorly with the proper lateral compartments of the bladder. Not infrequently the single or double cavity of the cæcum is partially subdivided internally by a series of circularly disposed, inwardly projecting ridges (*e.g.*, *Malapterurus*). In some Siluroids (*Pangasius*), where the lateral chambers are largely occupied by a trabecular network of fibrous bundles, the cavity of the posterior cæcum is largely obliterated by the formation of a similar growth. It may be remarked that in nearly all the Siluroids with an "elastic-spring-apparatus" that we have examined, posterior cæca were present, although in regard to their size and the extent to which their cavities are subdivided, or partially obliterated, considerable variety exists. In two species only (*Auchenipterus nodosus* and *Pangasius micronema*) are these structures entirely absent.

In all the normal Siluroids, without an exception, a ductus pneumaticus is present and opens into the anterior chamber in the median line of its ventral wall, and immediately in front of the ventral margin of the primary transverse septum.

Not only is the anterior compartment of the air-bladder more or less completely invested by bone on its dorsal and anterior surfaces, but its walls are attached to rigid portions of the axial skeleton and to movable ossicles at certain special points. As to the nature and extent of the fixed skeletal attachments, there is substantial uniformity in the different members of the group, and the physiological effect of such skeletal connexions is, in the great majority of cases, the same, viz., to render the anterior, dorsal, ventral, and posterior walls incapable of participating in any distension of the chamber, which, consequently, must solely depend upon the movements of the lateral walls. The posterior wall, *i.e.*, the primary transverse septum, is always attached by its dorsal margin to the ventral and lateral surfaces of either the complex or the fifth centrum—rarely to the sixth centrum; laterally to this the dorsal edge of the septum is invariably attached to the ventral surfaces of the transverse processes of the fifth vertebra, or to those of the fourth vertebra, or exceptionally to the corresponding processes of the sixth vertebra; and, in addition, a sheet of fibres is generally prolonged forwards, on either side of the complex centrum, into the dorsal wall, where it eventually becomes attached to the radial ridge of its side. We propose to speak of these attachments as forming the "posterior pillars" of the compartment.

As the anterior wall is usually more or less efficiently buttressed by the transverse processes of the fourth vertebra, or by post-temporal plates, or median subvertebral processes, the extent of its attachment to the skeleton varies inversely with the extent to which it is invested or supported by bone. The median portion of the wall is always attached dorsally to the ventral surface and sides of the anterior portion of the complex centrum, often by means of laterally situated, oblique, bony ridges, and also to the radial nodules. Laterally to this, on each side, the anterior wall may be so completely invested by bone as to be free from any special connexion or attachment to rigid portions of the axial skeleton (*e.g.*, *Macrones*); or in correlation with a less complete bony support, the outer stratum of the tunica externa of the anterior wall may separate dorsally from the inner stratum and become firmly inserted into the decurved anterior margin of the transverse process of the fourth vertebra (*e.g.*, *Arius*, *Auchenaspis*, *Pimelodus*). The dorsal attachment of the median portion of the anterior wall to the radial nodules and the complex centrum occurs in all the normal Siluroids, and may be regarded as constituting the "anterior pillars" of the compartment. The ventral wall may also be considered as rigidly attached to the skeleton, both in front and behind, inasmuch as its inner stratum of longitudinally disposed fibres, sometimes thickened into stout inwardly projecting ridges, extends into both the anterior and posterior walls, and shares the skeletal attachments of the anterior and posterior pillars. Although, as a rule, extremely thin, the median portion of the dorsal wall, over an area bounded in front and behind by the anterior and posterior pillars, and laterally by the dorsal walls of the two bulging halves of the chamber, is always firmly attached to the ventral and lateral surfaces of the complex centrum, and possibly also to those of the fifth centrum.

The attachment of the walls of the anterior chamber to moveable ossicles (the tripodes) is effected by the convergence of the fibres of the anterior and lateral walls into the dorsal wall in the form of two triangular sheets, and their ultimate insertion into the crescentic processes of the tripodes, which are situated near the anterior and inner corners of the lateral halves of the anterior chamber. The variations in the extent to which these fibres are attached to the tripodes are mainly confined to one feature. A slip of fibres derived from the median portion of the anterior wall is always inserted dorsally into the ventral ridge of each tripus, or directly into the ventral surface of the ossicle when the ridge fails to be developed. Laterally to this point the fibres forming the whole thickness of the tunica externa of the anterior and lateral walls may converge in the dorsal wall and become attached to the tripodes (*e.g.*, *Macrones*); or as in many other Siluroids (*e.g.*, *Arius*, *Pimelodus*, &c.) the outer stratum of the anterior wall is continuously attached by its dorsal

edge to the transverse process of the fourth vertebra, and only the comparatively thin inner stratum, in addition to the fibres of both strata from the lateral walls, extend into the dorsal walls and constitute the triangular sheets. In the latter case but few, if any, of the fibres of the inner stratum reach the tripodes, which, in consequence, only receive the direct insertion of the outer stratum of the tunica externa of the lateral walls.

Radial fibres arising from the radial nodules and inserted into the concave inner margins of the crescentic processes of the tripodes are invariably differentiated from the dorsal wall of the anterior chamber. In one instance (*Auchenipterus*), where the function of the radial fibres is taken by the flexible and highly elastic articular process of the tripus, the former are but scantily and feebly developed.

As we have previously pointed out, the lateral compartments of the air-bladder are neither invested by bone nor are they directly attached to the skeleton, but project freely into the anterior portion of the abdominal cavity. The most important feature in connexion with their structure, apart from their relatively greater capacity when compared with the anterior chamber, is their separation by a common longitudinal septum and the frequently septate condition of their cavities. Physiologically, the longitudinal septum and the secondary transverse septa subserve the double function of rendering the lateral chambers almost incapable of distension, and at the same time diminishing their susceptibility to the effects of external pressure.

Although we have never been able to detect the presence of intrinsic muscular fibres in the walls of the air-bladder, powerful extrinsic muscles are present in several Siluroids. In *Platystoma tigrinum*, *Pimelodus maculatus*, *P. ornatus*, and *Piramutana piramuta*, a powerful muscle takes origin from the posterior face of the skull, on each side of the foramen magnum, and is inserted into nearly the whole extent of the corresponding half of the ventral surface of the anterior chamber. As the contraction of these muscles must forcibly compress the anterior chamber we shall call them the "compressor muscles" of the air-bladder. They probably occur in many other Pimelodinæ, but, so far as our investigations are concerned, are probably confined to that group.

The presence of compressor muscles is invariably associated with the existence of a pair of much smaller muscles which arise from the exoccipitals, and are inserted into the anterior wall of the anterior chamber of the bladder. The tendon of each muscle has its insertion into the anterior wall immediately external to the complex centrum, and the insertion coincides with the extension of a slip of fibres from the inner surface of the anterior wall to the ventral ridge and concave inner margin of the crescentic process of the tripus. As the contraction of these muscles must evidently have the effect of limiting the

violent excursions of the tripodes which might otherwise take place when the anterior chamber is forcibly compressed by the contraction of its compressor muscles, we would suggest for each the name of "tensor tripodis."

An "elastic-spring-apparatus," provided with powerful protractor muscles, has already been described by Johannes Müller as existing in the South American genera *Auchenipterus*, *Doras*, and *Euanemus*, and in the African Siluroids *Synodontis* and *Malapterurus*. To this list our investigations enable us to add the South American form *Oxydoras brevis*, and the East Indian species *Pangasius buchanani*, *P. djambal*, *P. juaro*, and *P. macronema*. The absence of this mechanism in one species of Pangasius, viz., *P. micronema*, while present in all the remaining species of the genus that came under our notice, is an interesting and noteworthy fact.

In almost all normal Siluroids the lateral or outer walls of the anterior chamber of the air-bladder are more or less extensively and intimately applied to lateral cutaneous areas, and this relation of the two structures is always brought about by the divergence of the dorso-lateral and ventro-lateral muscles of the trunk, combined with the lateral extension of the anterior portion of the bladder.

(II.) *Siluridæ Abnormales.*

Omitting for the present all reference to such extremely aberrant forms as *Hypothalmus*, *Rhinelepis*, and the various Loricaroid Siluridæ, we confine our summary of this group to the various genera and species that have come directly under our notice. These are:— *Clarias*, *Saccobranchus*, *Eutropiichthys*, *Cryptopterus* (two species), *Ailia*, *Schilbichthys*, *Silondia*, *Acrochordonichthys*, *Akysis*, *Pimelodus* (two species), *Bagarius*, *Glyptosternum*, *Amblyceps*, *Cetopsis*, *Callomystax*.

In all these forms the series of rigidly interconnected vertebræ includes only the first, the complex, and the fifth vertebræ, the sixth being almost invariably free. The rigid articulation of the anterior vertebræ to the skull is as marked in this group as in the preceding one, and is brought about by precisely similar means. The centrum of the first vertebra is usually somewhat more rudimentary than in the normal forms, and neither it nor the basioccipital or the complex centrum are ever provided with accessory articular processes. The complex vertebra has the same general characters as in the foregoing group. Radial ridges and nodules are generally but are not invariably present; exceptionally the radial ridge may have no connexion at its inner extremity with the complex centrum (*Clarias*, *Glyptosternum*), and when this is the case the radial nodule may be absent, or confluent with the inner extremity of the radial ridge and widely separated from the complex centrum (*Clarias*).

The most characteristic of the many skeletal modifications which are exhibited in this group is the formation of more or less complete osseous grooves or funnels for the partial or complete enclosure of the air-bladder. Such recesses are formed by the transverse processes of the fourth vertebra, either singly, or in conjunction with those of the fifth vertebra, and vary greatly in depth and in the extent to which they are surrounded by bone. They may be comparatively shallow and widely open on the ventral side, as in *Akysis*, *Bagarius*, and *Glyptosternum*; or may take the form of deep, transversely disposed grooves, contracted distally and somewhat expanded proximally (*Pimelodus sapo*, *Eutropiichthys*, and *Schilbichthys*); or they may partake of the nature of transversely arranged bony cylinders or funnels, open distally in the dry skeleton, but otherwise with more or less complete osseous walls (*Clarias*, *Callomystax*, and *Cetopsis*). The transverse processes of the fourth vertebra always form the dorsal and anterior walls of the recesses, and sometimes furnish, in addition, a posterior wall, or even incomplete ventral walls; rarely do they completely enclose tubular recesses (*Cetopsis*); more frequently the posterior walls are formed by the transverse process of the fifth vertebra (*Clarias*, *Callomystax*). Exceptionally, a slender, lateral, bony outgrowth from each of the longitudinal ridges bounding the aortic groove in the region of the complex centrum may become attached to the ventral wall of the corresponding lateral air-sac (*Glyptosternum*, *Bagarius*); or, as in one instance (*Clarias*), the outgrowths may be strongly developed and form no inconsiderable portion of the ventral walls of the two osseous funnels. Ventrally, the shallow recesses may be closed by a tough fibrous membrane which also invests the corresponding walls of the contained air-sacs (*e.g.*, *Bagarius*); and by the same means vacuities in the walls of the more complete bony funnels are entirely closed (*Clarias*, *Callomystax*). The formation of a horse-shoe-shaped recess by the transverse processes of the fourth and fifth vertebræ in conjunction with plate-like lateral outgrowths from the aortic ridges, which is open laterally and behind in the dry skeleton, occurs only in one genus (*Ailia*). In whatever way the osseous recesses or capsules may be formed they are almost always open laterally or distally in the dry skeleton, and closed by lateral cutaneous areas in the fresh specimen.

The condition of the air-bladder in this group is singularly varied, and in proportion to the bulk of the Fish is always extremely small. Many of its most characteristic features are clearly the results of atrophy and degeneration. The principal modifications appear to be due to the partial or complete suppression of the lateral chambers and the subdivision of the anterior chamber into two laterally situated cavities or air-sacs, either by the solidification of the mesial portion of the bladder or by more or less complete longitudinal constriction.

In all cases the atrophied bladder is partially or completely enclosed within osseous recesses. In one or two instances (*e.g.*, *Schilbichthys* and probably also *Eutropiichthys*) the air-bladder, although solid mesially, nevertheless retains in each half traces of its original and normal division into anterior and lateral compartments, but the extreme thickness of its walls, and the small size of its internal cavities, afford sufficient proof of its degenerate and functionless condition. Solidification of the central portion of the bladder may reduce the cavity of that organ to the condition of a circular canal of fairly uniform calibre, surrounding a massive central pillar (*Silondia*). Two species afford good examples of the extreme variability to which degenerate structures are liable. In one (*Ailia*), the bladder assumes the shape of a tubular horse-shoe, and is almost solid, except at its hollow forwardly curved cornua; in the other (*Pimelodus sapo*) the organ is solid mesially, and of its two pairs of forwardly curved lateral branches one only is hollow and receives the insertion of the tripodes. In one case (*Cryptopterus micropus*, and possibly also *C. hexaptera*) the bladder consists of two partially separated lateral sacs, but its degenerate character is betrayed by the partial obliteration of the cavity of the posterior half by a network of fibrous bundles.

The more frequent condition of the air-bladder in this group is in the form of two simple, pyriform or globose, thin-walled, laterally placed air-sacs, which are either quite distinct or connected by an intermediate tubular portion (*e.g.*, *Glyptosternum*, *Oetopis*, *Acrochordonichthys*, *Bagarius*, *Akysis*, *Olarias*, *Saccobranchus*, and *Pimelodus pulcher*).

The skeletal attachments of the air-bladder both to rigid portions of the skeleton and to movable tripodes exhibit much the same extent and kind of variation as we have already described in the case of the anterior chamber of the more normal Siluroids. In such forms as possess rudiments of lateral chambers and of transverse and longitudinal septa the attachments of the air-bladder to the vertebral column and its processes are quite normal, and even in many members of this group where those structures are entirely wanting, the skeletal attachments are in the main very similar to those of the normal Siluroids. Thus, in those cases in which the air-bladder is represented by two entirely distinct or mesially intercommunicating sacs, the dorsal attachment of the primary transverse septum is represented either by the skeletal attachment of the dorsal edge of the median portion of the posterior wall to the ventral surface and sides of the complex centrum (*Pimelodus pulcher*), or by a similar attachment of the corresponding margin of the posterior wall of each lateral air-sac to the transverse processes of the fourth or fifth vertebra (*Bagarius*, *Glyptosternum*, *Clarias*). The median and tubular portion of the air-bladder, when present, is always thin, and its firm attach-

ment to the ventral surface of the complex centrum evidently corresponds to the skeletally attached medio-dorsal portion of a normal anterior chamber (*e.g.*, *Pimelodus pulcher*). Whether this intermediate tubular portion be present or absent, the inner portion of the anterior wall of each air-sac is always attached to the contiguous lateral surface of the complex centrum, or to the radial nodule, or even to both, after the fashion of the anterior pillars of the normal bladder. Occasionally also, as in such normal forms as *Arius*, the outer stratum of the anterior wall of each air-sac may be dorsally connected with the decurved anterior margin of the transverse process of the fourth vertebra (*e.g.*, *Bagarius*, *Glyptosternum*).

As a rule, where the air-bladder is more completely surrounded by its osseous capsules, the rigid skeletal attachments of the former are not so obvious as in those cases in which the bony investment is but partial.

The relations of each lateral cavity or air-sac to the crescentic process of the tripus imbedded in its dorsal wall are almost precisely the same as in each half of the anterior chamber in a normal Siluroid. The convergence of the fibres composing the anterior, lateral, and dorsal walls of each air-sac, and their insertion into the crescentic process of the tripus, takes place in nearly all the forms included in the present group, but in two or three instances (*e.g.*, *Glyptosternum*) the dorsal wall has partially or completely atrophied, or at all events has so far degenerated that it is extremely improbable that its fibres can possibly exert any pull upon the tripus as the result of any distension of the air-bladder.

Radial fibres are always present, but in some cases are less obviously specialised than in others, and, where radial nodules are absent, pass directly from the lateral surfaces of the complex centrum to the inner or concave margins of the crescentic processes of the tripodes.

Broadly speaking, it may be affirmed that the skeletal attachments of the air-bladder in the Siluridæ abnormales, both to rigid portions of the skeleton and to movable ossicles, are in substantial agreement with those of the anterior chamber of the *S. normales*.

The Weberian ossicles undergo but slight modifications in the different members of this group. Claustra are occasionally absent, and even when present are but feebly developed spicules of bone. As a rule, the scaphium has no ascending process, but only spatulate and condylar processes. The intercalarium may be absent, in which case the interossicular ligament is extremely short, or represented by a very small nodule in the usual position; horizontal and ascending processes are invariably wanting. The tripus is very variously modified. A ventral ridge is rarely present. The crescentic process may be curiously angulated and heeled (*Clarias*); nearly straight with a pointed posterior extremity (*e.g.*, *Bagarius*); or but slightly

curved (*e.g.*, *Glyptosternum*). The articular process is usually long and tapering.

Lateral cutaneous areas are almost invariably well marked and, as a rule, close the distal openings of the osseous recesses in which the air-bladder is lodged, but their relations to the outer walls of the latter vary considerably. In some forms, the structures are in close contact (*e.g.*, *Callomystax*), but more frequently they are separated by a considerable interval which is usually occupied by the lateral lobes of the liver, or by the anterior end of the mesonephros, or even by both.

A ductus pneumaticus may be present or absent. It is usually present where the laterally situated air-sacs are connected by an intermediate tubular portion, but is absent whenever the two air-sacs are completely separated.

The genus *Cetopsis*, referred to above, is one of the genera of Siluridæ in which J. Müller denied the existence of an air-bladder. In a specimen of *C. candira*, however, we found a rudimentary air-bladder in the form of two small oval sacs. Each sac was not more than 6 mm. in length, and was enclosed within the slightly dilated proximal extremity of a tubular or flask-shaped recess, enclosed by the transverse process of the fourth vertebra. The anterior wall of the recess was perforated by a small foramen, near the complex centrum, through which the tripus passed from its attachment to the air-bladder to its connexion anteriorly with the scaphium. As *Cetopsis* is the last of the nine genera of Siluroids in which J. Müller affirmed the absence of an air-bladder about which any doubt remained, it is now not unreasonable to assume that an air-bladder and Weberian ossicles are universally present in the group.

Of the more general conclusions which the foregoing data seem to us to warrant, we shall not now do more than draw attention to the following:—

(I.) The air-bladder and the Weberian mechanism in the Siluridæ are reducible to a common fundamental type, which is perhaps not very dissimilar to that illustrated by the condition of these structures in such normal forms as *Macrones* or *Arius*. From such a type the extraordinary variations met with in different genera, or even in different species of the same genus, are readily derivable, such variations being in part due to increasing specialisation—the result of physiological causes—and partly to the effects of disuse and consequent degeneration.

(II.) The air-bladder exhibits a far higher degree of specialisation in its relation to the Weberian apparatus than in any other Ostariophyseæ, but this fact renders it specially liable to degeneration, when the necessity for the exercise of its special function has,

from any change of habit on the part of the Fish, ceased to exist; and hence the widespread degeneracy of that organ.

(III.) In all the Siluridæ normales the air-bladder is a rudimentary and more or less functionless structure, and the numerous modifications which it presents in this group afford abundant illustrations of the extreme variability to which all degenerate organs are liable.

(IV.) As far as the evidence at our command will enable us to generalise, it seems extremely probable that the degeneracy of the air-bladder in the S. normales is due to their assumption of a ground habit, whereby the continued existence of an air-bladder, capable of functioning as a hydrostatic apparatus, is rendered unnecessary.

(V.) That inasmuch as the assumption of a ground habit is almost invariably attended by degeneration of the air-bladder, which must have the effect of rendering the Weberian apparatus inoperative, it seems to us a reasonable inference that the mechanism in question is related neither to the function of audition as Weber contended, nor to the appreciation of varying atmospheric pressures, as suggested by Sagemehl, but rather to the perception of the varying hydrostatic pressures to which the Fish is continually exposed. (Hasse's theory).

(VI.) Certain facts appear to throw some light on the nature of the Weberian ossicles. The discovery of ascending processes to the intercalaria, which form part of the wall of the neural canal, and are interposed between the foramina for the exit of the second and third special nerves, is confirmatory of the view, first suggested by Baudelot and supported by Ramsay Wright, that the ossicles in question represent the metamorphosed neural arch of the second vertebra. The mode of origin of the tripodes in *Auchenipterus*, if not due to secondary fusion with the arch of the complex vertebra, but to the retention of a primitive continuity, is also confirmatory of the views of the same morphologists, that those ossicles represent the transverse processes of the third vertebra.

VIII. "The Chemistry of the Urine of the Horse." By FRED SMITH, M.R.C.V.S., F.I.C., Army Veterinary Department, Professor, Army Veterinary School, Aldershot. Communicated by Sir WILLIAM AITKEN, F.R.S. Received June 20, 1889.

I have attempted in the following paper to record the results obtained from a series of analyses of the urine of the horse in health.

When I first commenced my subject I was under the impression

that, with the exception of the following references,* the literature of the subject was remarkably bare. It is true that nothing had been done in England, but on the Continent, in Germany in particular, the urine of the horse has received especial consideration. My attention was later called to the following references.†

My difficulty at starting was to obtain the whole twenty-four hours' urine; for this purpose I constructed a stall with sides which sloped towards the centre; running down the centre was a covered drain, the cover being perforated, and arranged in segments so as to allow of thorough cleaning; this drain led to the rear of the stall, and emptied into a vessel sunk in the ground suitably protected against ingress of foreign material. The entire apparatus was made in cast iron, and protected against rust.

The arrangement was found to give absolute satisfaction.

This plan of collecting the urine is nothing like so complicated as that used by Munk and others in Germany, which consists of a bag into which the penis is placed, the bag being secured by numerous straps around the belly and between the thighs. There are very few English horses which would allow such an apparatus as Munk's to be strapped under the belly. I shall use this appliance to collect the urine from sick animals, for it is likely that these will not object to wear Munk's contrivance.

The horse to be experimented upon was previously weighed, when considered necessary, and was then placed in this stall and tied up for twenty-four hours; the stall was made very narrow so that the animal could not possibly shift from his position. To keep the fæces out of the drain, a little clean straw was put down.

One great object I had in view in making these experiments was to ascertain the difference between the urine of work and that of repose. The only way in which I could get approximate results with regard to the former was by working the animal for one or more days, and then collecting the urine for the last twenty-four hours; I always took the precaution of ascertaining in every case the urine of repose after at least two or three days' rest.

The total number of complete analyses made was fifty-four, and these extended over a period of two years; the total number of urines examined was ninety-six. Influence of season, work, diet, sex, age, &c., were most carefully observed. None of my results were

* 'Animal Chemistry' (Liebig); Thomson's 'Animal Chemistry' (Fourcroy and Vauquelin); Colin's 'Physiologie Comparée' (Boussingault); 'Phil. Trans.,' 1806 (Brande); 'Physiological Chemistry' (Lehmann); Simon's 'Chemistry.'

† Salkowski, 'Zeitschrift für Physiologische Chemie,' vol. 9, 1885; Munk, 'Archiv für Anatomie und Physiologie, Physiol. Abth.,' 1880, Suppl.-Heft; Tereg and Munk, *ibid.*; O. Kellner, 'Landwirthschaftliche Jahrbücher,' vols. 8 and 9; Siedamgrotzky and Hoffmeister, 'Elémentsd'Analyse Chimique;' J. Tereg, 'Encyklopädie der gesammten Thierheilkunde und Thierzucht' ("Harn").

calculated until the inquiry was completed; it was then observed that the composition of healthy horse's urine will vary within wide limits, and that even from day to day the same horse will excrete a fluid of very varying composition, though his condition of diet, &c., remains absolutely the same. I am not prepared at present to offer an explanation of this condition, which so seriously affects my tables of mean results as to render them only approximately true.

Physical Characters of the Urine.

Turbidity.—The normal urine is invariably turbid, due to the suspension of the carbonates of lime and magnesia which precipitate themselves in still greater abundance as the urine cools and stands, and undergoes ammoniacal fermentation.

The amount of salts in suspension is in some cases remarkable, the most common being the carbonates of lime and magnesia, which I have in the majority of my analyses estimated separately as suspended lime and magnesia. Boiling the urine by driving off CO_2 precipitates more of the lime salts. In one or two cases after the urine had stood some days, a hard scum, quite crystalline, has formed on its surface; this has consisted of crystals of lime carbonate. Only once in ninety-six observations had I a perfectly clear urine presented for examination, a urine which threw down no deposit on cooling and standing, and was in most of its physical features closely allied to human urine.

Smell.—Perfectly fresh urine has a faint but distinctly ammoniacal smell; the fluid which represents the twenty-four hours' excretion is always powerfully ammoniacal. This latter creates a difficulty with regard to the determination of urea, for it is impossible to say how much of the ammonia is due to changes in the urea, and how much is preformed ammonia. I have always felt this a trouble throughout the work, but will later explain how I have endeavoured to overcome it.

Reaction.—This is always alkaline, sometimes faintly, but in the majority of cases strongly so. The alkalinity shown by test papers is produced by a fixed and by a volatile substance—the volatile is the ammonia, the fixed is probably a salt of potash. It is obvious that the amount of volatile alkalinity present depends greatly upon the time of year, the condition of the urine (those containing most mucus containing most volatile alkalinity), and the length of time which has elapsed before the estimation is made. As remarked in the last paragraph, how much of the volatile alkalinity in urine twenty-four hours old is due to preformed ammonia, and how much to the ammonia formed at the expense of the urea, it is difficult to determine; it is probable, however, that the preformed ammonia in urine is given

off in twenty-four hours. Calculated as ammonia, the mean volatile alkalinity in twenty-four hours old urine amounts to 7·1016 grams for work and 7·8534 for rest; these amounts I have added on to the urea, as I am convinced from long observation that they are formed from this substance. The fixed alkalinity expressed in terms of KHO gives a mean of 2·954 grams in urine twenty-four hours old, but in perfectly fresh urine it is equivalent to 4·8856 grams of KHO in twenty-four hours; this latter is probably too high.

Consistence.—A large quantity of mucus in the urine is by no means an uncommon condition; this is particularly the case in mares, the urine being so thick and tenacious (more like linseed oil in consistence) that it takes some hours to get sufficient through the filter for analysis.

The smaller the bulk of fluid excreted, the larger the amount of mucus it contains; it then becomes sticky and difficult to work with, still, it is a perfectly natural condition. In a urine of average consistence I have found 21·9 grams of mucus, and in one very tenacious 31·396 grams in twenty-four hours.

Specific Gravity.—The mean specific density is 1036, the highest registered was 1050 and the lowest 1014. The formulæ of Trapp and Christison will not apply to the urine of the horse. Solids calculated by these give untrustworthy results.

Quantity of Urine.—The mean amount of fluid excreted by working horses is 4474 c.c. and in animals that rest 4935 c.c. The largest amount produced in twenty-four hours was 11,300 c.c., and the smallest quantity secreted 2000 c.c. Neither season, sex, or age produced any effect on the quantity of fluid secreted. In thirteen observations on the same horse, embracing both hot and cold weather, the largest quantities passed, amounting to over 10 litres, were produced during warm summer months. I place, however, no stress on this observation; probably in another series of experiments the results would be reversed.

It is obvious that much of the bulk of fluid secreted will depend upon the quantity consumed. It is notorious that working horses are often stinted in their water. In one very careful experiment, where all the water was measured, it was found that more urine was excreted during the twenty-four hours subsequent to work than was excreted after absolute rest for one week. The water of the twenty-four hours' urine equals $\frac{1}{4}$ to $\frac{1}{5}$ of the water drunk.

Chemical Characters of the Urine.

Total Solids.—The mean amount of solids excreted by horses at rest was 230·0713 grams; of these the combustible solids are represented by 146·1649 and the ash by 83·9064 grams. The total solids of

work are 232·157 grams, the organic solids 152·190 grams, and the ash 79·967 grams. Great variation both at rest and work is observed in the total solids, even where the diet remains the same.

The nature of the diet, according to Tereg,* considerably influences the amount of the urinary solids excreted, as shown in the following table:—

Daily ration.			Solids in the urine.
Hay.	Oats.	Wheat straw.	grams.
8 kilos.	2 kilos.		566·6
7 "	2 "	1 kilo.	529·4
6 "	4 "		511·8
4 "	4 "	2 kilos.	477·0
4 "	6 "		460·7
1 "	6 "	2·6 "	346·1

About 90 per cent. of the ash is soluble in water, and 10 per cent. soluble in acid. In the watery solution of ash we find the chlorides of sodium and potassium, traces of lime, phosphates, magnesia, and sulphates. In the acid solution lime, magnesia, and sulphates predominate. On looking at the inorganic solids, they are smaller than I had expected; the extreme difficulty experienced in incinerating urinary solids causes, undoubtedly, a loss by the volatilisation of the chlorides, &c.

Urea.—In calculating the urea we have also to take into consideration the carbonate of ammonia which unavoidably forms during the twenty-four hours the urine is being collected.

To show how much of the urea breaks up owing to fermentation, I have calculated it separately in the table, and then added the two together. I used Liebig's method of determination for some time, but it gives too high results.

My most trustworthy observations have been made with the hypobromite process.

The influence of rest and work over the production has been most carefully studied.

I originally held the view that more urea was excreted during work than during rest, and a long series of analyses supported this view. I found, in fact, in tabulating my results that the resting horses excreted on an average 88·41 grams of urea daily, of which 13·778 grams were in the form of ammonia carbonate; whilst working horses excreted 134·9291 grams, of which 12·4591 grams existed as ammonia carbonate.

The incorrect conclusions which appeared forced on me were due to the fact that the excretion of urea, even on a fixed and rigid diet, is extremely variable, and in the horses from which the above results

* 'Encyklopädie der Gesammten Thierheilkunde,' vol. 4.

were obtained I failed to keep any of them long enough under observation to find out this point of variability.

Again, diet influences the production, as proved by the work of Tereg, Munk, and others, who have shown that on a hay diet more nitrogen is excreted than on one containing oats as well as hay; this seems so opposed to what one would expect that I overlooked the point entirely.

Tereg and Munk put down the amount of urea as 120 grams excreted daily in a horse weighing 400 kilos., the diet being oats (4·5 kilos.) and hay (2·5 kilos.). The mean of my own observations is 111 grams; but urea varies much, even on a fixed diet, in different horses; in Tereg and Munk's experiments it varied from 81·5 to 149·5 grams in twenty-four hours.

I experimented on a pony weighing 5 cwt. 21 lbs.; the experiments lasted from 2nd February to 29th March, and were divided into periods of rest and work; the diet throughout remained the same, viz., hay 7 lbs., oats 5 lbs.

In the first series of rest and work I found that the animal excreted, on an average, for the resting days 63·63 grams urea, and for the working days 72·913 grams.

In the second series of rest and work, all conditions remaining the same, I found more urea during the resting than during the working period, viz., for rest 65·125 grams, and for work 43·33 grams.

Urine of Rest.

	Total nitrogen.	Urea.
2nd February	28·56 grams.	60·0 grams.
4th ,, ...	35·20 ,,	62·5 ,,
6th ,, ...	32·00 ,,	68·4 ,,

The animal was now worked from the 7th until the 15th February, and again worked on the 17th and also on the 19th.

Urine of Work.

	Total nitrogen.	Urea.
16th February	32·48 grams.	63·0 grams.
18th ,, ...	47·71 ,,	81·37 ,,
20th ,, ...	42·63 ,,	74·37 ,,

Complete rest was now given until the 23rd February, when the experiments were repeated.

Urine of Rest.

	Total nitrogen.	Urea.
23rd February	44·60 grams.	84·0 grams.
26th ,, ...	57·10 ,,	80·0 ,,
1st March............	31·36 ,,	56·0 ,,
13th ,, ...	19·00 ,,	40·5 ,,

The animal was worked from the 14th until the 24th March, and again on the 26th and 28th March.

Urine of Work.

	Total nitrogen.	Urea.
25th March	16·324 grams.	30·25 grams.
27th "	25·725 "	52·25 "
29th "	23·000 "	47·50 "

The table clearly shows us how variable is the excretion of urea in spite of the fact that the diet remained the same; it is evident that the urea in horses is no more a measure of the muscular waste than it is in man. Kellner's experiments* are remarkably complete. He made horses produce a definite amount of work; the experiment was divided into five periods:—

	Work produced.	Nitrogen produced.
1st Period	475,000 kilogrammeters	99·0 grams.
2nd "	950,000 "	109·3 "
3rd "	1,425,000 "	116·8 "
4th "	950,000 "	110·2 "
5th "	475,000 "	98·3 "

Here we have a slight increase in the output of nitrogen, quite insufficient to account for the increased work produced.

Hippuric Acid.—Owing to the statement made by Liebig that benzoic acid was found in the urine of working horses, and hippuric in the urine of those which rested—a statement which has often been repeated since his time, and almost formulated into the doctrine that benzoic was present in the horses of the poor, whilst hippuric predominated in that of the wealthy—I have been at great pains to discover what element of truth the doctrine contained.

The method employed for the determination of benzoic and hippuric acids was the following:—

The urine is treated with excess of milk of lime, filtered, evaporated to one-fifth of its bulk, and acidified with HCl. If hippuric acid be present it forms in some cases almost immediately, but in the majority it has to stand from twelve to twenty-four hours; if benzoic be present it forms almost at once. Both acids are in a highly impure condition, the hippuric (in black seaweed-like masses) is dissolved in water, boiled, and, whilst boiling, a current of chlorine gas passed through it to destroy the organic matter; it is then filtered hot, and deposits pure hippuric acid in fine needles in the course of a few hours. The impure benzoic is filtered, the solid residue collected in

* 'Landw. Jahrbücher,' 1879.

a capsule, dried at a low temperature, and carefully volatilised, when beautiful white sparkling crystals form, which are carefully removed, collected, and weighed; or the impure mass may be dissolved in ether, the solution evaporated, and then volatilised. This volatilisation requires great care to avoid loss.

I have tried many methods of obtaining these acids, but none give such satisfactory results as the above.

The examination of the twenty-four hours' urine of fifty-four horses revealed the presence of hippuric acid on only eight occasions.

The number of horses at work was seventeen, and out of these I found hippuric acid twice, 2·144 grams and 18·6 grams respectively. The number of horses standing idle was thirty-seven; of this number I found hippuric acid six times; two of these observations I must deduct, as the horses were not in perfect health, leaving four out of thirty-five as the proportion in which hippuric acid was detected.

In a second series of observations consisting of thirty horses, the urine from which was collected and at once submitted to analysis, I found that out of eighteen working horses thirteen had hippuric acid in the urine and five had none. Out of twelve horses at rest three had hippuric acid and nine had none. The diet in all cases was the same. This would appear to reverse Liebig's theory.

My observations show that hippuric acid is generally found in the urine of working horses—seldom found in the urine of resting horses, and that it is rarely found in urine twenty-four hours old.

Diet influences the production of hippuric acid, and it is increased by using meadow-hay and oat-straw, and decreased by using clover, peas, wheat, oats, &c.; as the urea rises the hippuric acid falls, and *vice versâ.* (Tereg,* Weiske and Kellner.†)

The mean hippuric acid found was 15·58 grams, the maximum 28·56, and the minimum 9·18 grams in twenty-four hours.

Salkowski places the hippuric acid at 15·597 grams daily.

Benzoic Acid.—Benzoic acid is generally found in stale urine, and in the urine of horses which are doing no work. It may, however, be found in working horses, or a urine may possess neither hippuric or benzoic acids.

The mean benzoic acid found in resting horses was 6·53 grams, in those at work 3·62 grams in twenty-four hours.

Total Nitrogen.—In my earlier observations I believed that the nitrogen of work was greater than the nitrogen of rest.

I have explained under urea how I fell into the error, and I have there fully detailed the nitrogen during rest and work in a series of experiments on a pony. The nitrogen is as variable as the urea; in my earlier series it varied for horses between 46 and 70 grams per diem.

* 'Encyklopädie,' &c.

† Watts' 'Dictionary of Chemistry,' vol. 8, Part II.

Here diet undoubtedly influenced its production—as previously pointed out under the head of urea.

E. Salkowski states that a horse fed on 2 kilos. oats, 2 kilos. hay, and 1 kilo. bran, excreted 65·34 grams of total nitrogen in twenty-four hours.

According to Tereg and Munk, when horses are fed on rye instead of hay and oats the nitrogen shows no change, but by feeding with peas the nitrogen increases, and that in proportion to the quantity given. If fed on hay alone the excreted nitrogen is very great, a fact as pointed out by these observers, which is very difficult of explanation.

Ammonia.—This exists in the urine of horses free and combined; the latter has been dealt with and its origin explained, the free ammonia may or may not be due to fermentation occurring in the bladder, but from a very large number of observations on perfectly healthy horses I affirm that ammonia exists in a free state in fresh urine.

It may be that ammoniacal fermentation has already taken place in the bladder due to the quantity of mucus, and the long period during which the majority of horses retain their urine, due both to habit and circumstances, but it is quite certain that the perfectly fresh urine caught directly into clean vessels contains a distinct amount of ammonia. The amount of this ammonia cannot be estimated in urine twenty-four hours old, because it is impossible to distinguish it from the ammonia formed as the result of urea decomposition.

The only way I have attempted to overcome the difficulty is by collecting perfectly fresh urine, and by Schlösing's method determining the ammonia before the slightest urea change, outside the body, has occurred. This process is far from being free from error, but is the least objectionable mode of procedure.

I have previously stated that the ammonia found in urine twenty-four hours old may safely be calculated as urea, for that is undoubtedly its origin.

The preformed ammonia is probably completely given off before the twenty-four hours have ended. The amount of free ammonia in the urine of rest I have calculated at 2·516 grams, and in the urine of work 5·3 grams, but I do not regard these results as completely trustworthy.

They nevertheless agree very closely, particularly that of work, with the ammonia obtained by the direct titration of fresh urine with a standard acid.

Phosphoric Acid.—This acid is only found in comparatively small quantities in the urine of horses, the phosphates being principally eliminated by the bowels.

According to Boussingault, horses do not excrete phosphoric acid; this is not in accordance with our experience.

Diet possesses no influence over its production, and the effect of rest and work is insignificant. I found that working horses excreted 1·897 grams as a mean, whilst horses at rest excreted 1·3 grams. Age has no influence over its production. The largest amount found was 9·45 grams and the smallest 0·13 gram.

The amount of P_2O_5 will vary very considerably, many horses only just possessing traces of the acid, others distinct quantities. I am inclined to regard the mean amounts of phosphoric acid given above for rest and work as rather high.

Sulphuric Acid and other Sulphur Compounds.—Sulphur exists in two distinct forms in the urine of horses; the one I have calculated as SO_3, the other, known as sulphur compounds, is calculated as S. Diet appeared to have no influence in the production of SO_3, work, on the other hand, increased it. Working horses excreted on an average 15·289 grams, and horses at rest 10·6468 grams in twenty-four hours. The SO_3 appears to be increased in working horses in the same proportion as the urea.

The sulphur compounds are said to exist in combination with phenol and other organic substances; on this point I am not prepared to offer any opinion.*

Work did not influence their production. Working horses yielded 7·6092 grams, horses at rest 7·3166 grams of sulphur. It is singular that horses should excrete so much sulphuric acid and other sulphur compounds.

Chlorine.—More chlorine is excreted during rest than work, the mean amount for the former being 31·7119 grams, and for the latter 21·9806 grams in twenty-four hours.

The chlorine is not affected by diet; it is united with potassium and sodium; the amount of the latter metal in the urine of the horse is small, and only yields with the chlorine about 5½ grams of NaCl daily; the major part of the chlorine is united with potassium which is most abundant.

* Some excellent work, has, however, been done in this direction by Salkowski, Tereg, and Munk. The latter observers state that on an average horses excrete 10·886 grams of tribromphenol in twenty-four hours, 10·175 grams of inorganic sulphur, and 5·039 grams organic sulphur in twenty-four hours. The tribromphenol is equivalent to 3 grams of phenol daily. Great importance is laid by these observers on the excretion of phenol, a process which is suspended during intestinal complaints, particularly colic, and is, according to them and others, a cause of the rapid death in these affections, produced by the toxic effect of the unexcreted phenol. The production of phenol in the healthy body is greatly influenced by diet, being largest on rye and hay, one part peas and two parts oats, and on hay alone; it is smallest on rye alone, and next smallest on oats and hay. Salkowski is inclined to regard Tereg and Munk's estimate of 3 grams of phenol daily as too high.

Lime.—More lime exists in the urine of the horse than is soluble in an alkaline fluid, we have therefore lime both dissolved and merely suspended; these have been estimated separately. No direct connexion could be traced between the lime in the urine and the lime in the food, but between the production of lime and work a direct connexion appeared. The mean amount of dissolved CaO at work was found to be 1·9027 grams, and of the same salt at rest 3·4367 grams in twenty-four hours.

Suspended Lime.—More suspended lime was found in the urine of horses at work than in those at rest, for the former 3·69 grams, and for the latter 1·1043 grams CaO. To state these points briefly, when horses work they excrete more lime in their urine than when at rest, but the lime of work is principally suspended, and only a part of it dissolved; whereas, the lime of rest is nearly all dissolved, and but little of it suspended. There is no connexion between the amount of mucus in the urine and the suspended lime.

The largest amount of dissolved lime I found was 16·45 grams and the smallest 0·627 gram in twenty-four hours. The lime is found principally in conjunction with a carbonate, but I have also found sulphate and oxalate. The most common deposit in horse's urine is the wheel-shaped crystals of lime carbonate.

On adding an acid to urine, extreme effervescence occurs as a rule, and the fluid is left quite clear like human urine; I have only on a few occasions witnessed any different results from these. The effervescence is usually extreme.

Magnesia.—This, like the lime, exists partly in the suspended state and partly dissolved. Neither diet nor work have any influence over the production of magnesia. The soluble magnesia of work is 2·63 grams, and of rest 2·975 grams. The suspended magnesia of rest is 0·4218 gram, and of work 0·7925 gram.

Potassium.—This metal is found largely in horse's urine; it is principally combined with chlorine.

Rest and work influence its production, there is more potash found in the urine of resting than in the urine of working horses. Working horses gave 27·06 grams whilst resting horses gave 36·59 grams in twenty-four hours.

Sodium.—Is only found in small quantities in the urine, the mean amount being 2·17 grams, and it is combined with chlorine, yielding a little over 5½ grams of common salt for the twenty-four hours. The mean amount of sodium in working horses was 1·84 grams in twenty-four hours, in horses at rest it was larger, viz., 2·5 grams; yielding with chlorine less than 6½ grams of common salt per diem.

In some recent experiments, carried out on a pony, on the excretion of soda and potash during rest and work, the animal remaining under observation for several days, I found that the mean amount of

mixed chlorides excreted remained about the same, viz., 40 grams daily both at rest and work, but that during rest more potash and less soda was found than during work. This does not agree with the above experiments on the horse.

I have compiled, from the mean result obtained, the following table of analyses of urine of healthy horses at work and rest.

I observe that my inorganic solids of rest are in excess of the ash found, and moreover, that the amount of organic substances found in the urine of rest is much smaller than that obtained by evaporation and weighing. In both these matters the urine of work gives better results.

It is obvious, however, that I have only dealt with the most common and important substances found in urine; there are many organic substances which I have not looked for, or estimated, or have only estimated on so few occasions as not to entitle them to a place in the table.

I do not for one moment intend it to be supposed that the table represents what all horses at work or rest excrete, for I have previously stated that the horse's urine is a fluid of *very varying composition;* all the table represents is the mean of a large number of carefully made observations, which must be accepted as an approximation to the truth rather than as absolutely true.

Table showing the Mean Composition of the twenty-fours' Urine of Horses at Rest and Work.

	Rest.	Work.
	c.c.	c.c.
Quantity	4935	4474
Specific gravity	1036	1036
	grams.	grams.
Total solids	230·0713	232·157
Organic solids	146·1649	152·190
Inorganic solids	83·9064	79·967
Urea	98·5110	
Ammonia carbonate as urea	13·1185	
Ammonia	2·516	5·3000
Benzoic acid	6·530	..
Hippuric acid	..	15·5870
Phosphoric anhydride	1·3000	1·8970
Sulphuric anhydride	10·6468	15·2890
Other sulphur compounds	7·3166	7·6902
Chlorine	31·7119	21·9806
Calcium oxide	3·4367	1·9027
Magnesium oxide	2·9750	2·6300
Potassium oxide	36·5900	27·0600
Sodium oxide	2·5000	1·8400

I have made no mention of the changes which occur in the urine as the result of disease, for the reason that I purpose devoting a special article to this subject, which is at present under investigation, though necessarily slow in progress, and far from being completed.

It is singular to observe that any important derangement of the horse's health is associated with an acid urine, the presence of uric acid and large phosphates, and the production of a clear human-like urine in appearance; this change is produced as soon as the animal refuses food and commences to live on its own tissues.

IX. "A Chemical Inquiry into the Phenomena of Human Respiration." By WILLIAM MARCET, M.D., F.R.S. Received June 3, 1889.

(Abstract.)

Before entering upon this communication, I must beg to acknowledge the valuable aid of my assistant, Mr. C. F. Townsend, F.C.S., to whose diligent, methodical, and careful work I am greatly indebted for the results obtained in the present research. The numerous calculations have all been made by both of us together, and the results checked in every possible way to insure accuracy.

My attention was first turned to the chemical phenomena of respiration in 1875, and since then I have had the honour of communicating to the Royal Society a succession of papers on the "Influence of Altitude on Respiration," which have appeared in vols. 27, 28, 29, and 31 of the 'Proceedings.'

These inquiries show in a most conclusive manner that altitude exerts an action on respiration depending entirely on the fall of atmospheric pressure. The law can be expressed as follows:—The volumes of air breathed, reduced to 0° C. and 760 mm., in order to yield the oxygen necessary for the production of a given weight (say, 1 gram) of carbonic acid, are smaller on mountains under diminished pressures than in the plains under higher pressures.

My earliest experiments on the Breithorn, 4171 metres (13,685 feet); the Col St. Théodule, 3322 metres (10,899 feet); the Riffel, 2368 metres (8428 feet); St. Bernard, 2473 metres (8115 feet); and the Col du Géant, 3362 metres (11,030 feet), were all attended with a fall of temperature on reaching into higher altitudes. This circumstance necessarily produced an increased combustion in the body, to overcome the action of the cold, and introduced an element in the inquiry not unlikely to interfere with the exclusive influence altitude might exert on the chemical phenomena of respiration. In order to overcome the present difficulty I spent three weeks on the Peak of Teneriffe in the summer of 1878, where the

experiments were repeated. The temperature, though varying to a slight extent at different altitudes on the Peak, was always high in the daytime; hence there was no cause for any increased formation of carbonic acid in the body towards the resistance of cold. The result was most striking. While in the cold Swiss Alps I had observed an increased expiration of carbonic acid in ascending, on the Peak of Teneriffe there was no such effect produced. The mean weight of carbonic acid expired at the three stations, by two persons, was, with one exception only, applying to a Chamonix guide, the same for each of them respectively. But the volumes of air breathed at increasing altitudes were lessened,* so that the law remained unchanged—that at increasing altitudes, less air, reduced to 0° C. and 760 mm., is required to produce 1 gram of carbonic acid in the body. The experiments on the Peak of Teneriffe, by doing away entirely with the influence of cold, place the fact beyond doubt.

As it is important towards a clear understanding of the results contained in this paper that those obtained formerly should be present to the reader, I beg to subjoin them in a tabular form.

Experiments on the Alps.

(On myself.)

Station.	Altitude.	Litres of air expired for 1 gram CO_2, reduced to 0° and 760 mm.
Near Geneva	375 m. (1,230 ft.)	13·6
St. Bernard	2473 „ (8,115 „)	Mean at and above 8,115 ft. up to 13,685 ft. 11·05
Riffel	2568 „ (8,428 „)	
St. Théodule	3322 „ (10,899 „)	
Summit of Breithorn	4171 „ (13,685 „)	

On the Peak of Teneriffe.

(On myself.)

Seaside		12·4
Guajara	2161 m. (7,090 ft.)	11·9
Alta Vista	3261 „ (10,700 „)	10·7
Foot of terminal cone	3578 „ (11,740 „)	10·6

On the Col du Géant.

(On myself.)

Near Geneva	375 m. (1,230 ft.)	15·5

* An exception again for the Chamonix guide at his highest station, although the *mean* reduced volume of air he expired at the two high stations on the Peak is lower than that he expired at the seaside by 18 per cent.

			Litres of air expired for 1 gram CO_2, reduced to 0° and 760 mm.
			Mean before and after ascent.
Courmayeur	1202 m.	(3,945 ft.)	14·35
Summit Col du Géant	3362 „	(11,030 „)	13·5

On M. David, aged 25.

Near Geneva	375 m.	(1,230 ft.)	13·7
			Mean before and after ascent.
Courmayeur	1202 „	(3,945 „)	14·8*
Summit Col du Géant	3362 „	(11,030 „)	12·6

The experiments were made by determining as carefully as possible the volume of air expired within a given time, and then estimating the amount of carbonic acid it contained by means of Pettenkofer's method. The volume of air, reduced to 0° C. and 760 mm. pressure, holding 1 gram of carbonic acid, was then easily calculated.

These experiments were subsequently repeated in 1882 on the Rigi Mountain in Switzerland, altitude 1594 metres (5230 feet), and as the results obtained have never been published, beyond a short reference to them in a communication to the Alpine Club, they are included in the present paper. My companion, Mr. Thury, a young engineer, aged twenty-five, submitted to the inquiry. Fifteen experiments were made near Geneva at a mean barometer pressure of 728 mm., and a mean temperature of 15·9° C., and eighteen as soon afterwards as possible on the Rigi Staffel, at a mean pressure of 639 mm., and a mean temperature of 7·6° C. As might have been expected, more CO_2 was exhaled in a given time on the cold mountain station than in the Valley of Geneva, a mean of 0·350 gram being expired near Geneva and 0·445 gram at the higher station, giving an excess of no less than 21 per cent. of carbonic acid for the Rigi. The amount of air expired—say breathed—for the expiration of 1 gram CO_2, reduced to 0° and 760 mm., was 10·78 litres in the Valley and only 9·45 on the Rigi. Therefore, for a mean difference of 89 mm. of atmospheric pressure, and with a marked fall of temperature, less air by 12 per cent. was breathed on the mountain to supply the oxygen required by the body to burn the same amount of carbon as in the valley.

The present investigation has been carried out in a laboratory of the Physiological Department at University College, which Professor Schäfer has kindly placed at my disposal. Its object was to ascertain

* This is an exceptional case, owing clearly to the small increase of altitude between Geneva and Courmayeur, which only amounts to 827 metres.

the influence of food and changes of atmospheric pressure on the volume of air breathed and weight of CO_2 expired. Two persons, my assistant, Mr. C. F. Townsend, and my laboratory attendant, William Alderwood, kindly submitted to experiment.

The person under experiment sat in a semi-recumbent posture in a deck-chair, with his feet resting on a stool, so as to do away with all muscular effort. He inspired by the nose only, and expired through the mouth into a wide india-rubber tube, connected with a bell-jar, of a capacity of over 40 litres, and suspended over salt water. The bell-jar was accurately counterpoised over a pulley fixed to a cycloid, whose leverage power, increasing as the bell-jar rose, kept the latter perfectly balanced, and therefore the air it contained was under atmospheric pressure in every position. The time for collecting the air expired was measured with a stop watch. In order to make sure of no air being expired accidentally through the nose, at the end of each inspiration the nose was closed with the index-fingers, and thus held during the expiration.

In order to obviate the objection that the attention given to the experiment might interfere with natural breathing, the air was expired into the bell-jar through a double-way cock, disposed in such a manner that the person under experiment might, unknown to him, either expire into the external air or into the bell-jar. At the commencement of the experiment he was made to expire into the open air, and when, after ten minutes or a quarter of an hour, his respiration had become perfectly regular, the stopcock was turned and the air admitted into the bell-jar. The latter was so well suspended that it rose without the least effort; thus, the person experimented upon, unless looking at the bell-jar, could not tell whether he was breathing into it or into the external air.

The carbonic acid was determined by aspiring with a pump the air from the bell-jar into a glass cylinder of a capacity of 1000 c.c., to which was subsequently screwed, air-tight, a bottle holding 100 c.c. of a normal solution of barium hydrate. After agitating the air with the akaline solution for a minute or two, about 100 c.c. of common air free from CO_2, and contained in a pear-shaped india-rubber bag, was forced into the cylinder by pressure with the hand, and then the shaking resumed for a quarter of an hour. The addition of the air caused a pressure inside the cylinder which was found to accelerate greatly the combination of the CO_2. Finally, the alkaline solution was decanted into a glass-stoppered bottle of a capacity of about 100 c.c., and the stopper secured with paraffin. The morning of the next day, when the precipitate had entirely subsided, the clear fluid was titrated with a standard solution of oxalic acid in the usual way.

A number of precautions were taken to insure the accuracy of the method; perhaps the most important was blowing, with a bellows, a

current of air over pumice-stone moistened with a solution of potassium hydrate, through the wide mouth bottle in which the titration was being made; by this means no atmospheric carbonic acid could interfere with the correctness of the result. Fourteen pairs of analyses, made to test the method, gave a mean difference of only 0·31 per cent.

Two comparative experiments were carried out in a large air-tight chamber in which a person lying in a deck-chair breathed first into an india-rubber bag, representing the bell-jar, and next into the air of the chamber. The air in the bag and in the chamber being subsequently analysed yielded practically the same weight of carbonic acid expired within the same time.*

The results obtained from the present inquiry are as follows:—

1. The law of nature is further demonstrated that less air, reduced to 0° C. and 760 mm. pressure, is breathed at high than at low altitudes for the formation in the body of a given weight of carbonic acid.

2. The known usual influence of food on the formation of carbonic acid in the body is confirmed—the maximum amount expired occurring between two and three hours after a meal, while the minimum is before breakfast.

3. The influence of food on the relation between the volumes of air breathed (reduced) and the corresponding weights of carbonic acid expired is clearly shown; the volumes following, as a rule, the fluctuations of the carbonic acid, but there is apparently a sudden change in this relation at a period of between four and five hours after a meal, when the carbonic acid expired falls proportionally faster than the volumes of air breathed. The harmony of the tracings in one of the charts accompanying my paper has recovered itself, however, overnight, and the lines are again nearly parallel before the first morning meal. In the other chart there are no experiments recorded made before breakfast.

4. The local state of the atmospheric pressure, as shown by the barometer, has a marked influence on respiration, less air, reduced to 0° C. and 760 mm. pressure, being taken into the lungs for the formation and emission of a given weight of carbonic acid under *lower* atmospheric pressures than under *higher* pressures; but this influence varies in degree according to different persons. In the present inquiry when two young men were experimented upon—in one case, for a fall of pressure of 10 mm. (0·395 inch), there was a mean reduction of 1·076 per cent. of the volume of air breathed for 1 gram CO_2 expired; in the other case, the mean reduction was greater, and amounted to 1·745 per cent.

5. The above influence of local atmospheric pressures on the volume of air breathed is not the same throughout the whole day, being much less marked from two to four hours after a meal when the action of

* In one experiment the difference amounted to 2·97 per cent.; in the other to 0·6 per cent. only.

food is at its maximum. Thus digestion neutralises in a great measure the effects on respiration of any local change of pressure.

Two persons, both aged twenty-three, were found in the experiments related in this paper to require respectively a mean of 9·29 and 10·51 litres of air, reduced, to expire 1 gram carbonic acid. Experiments on another person, aged sixty, gave a mean of 11·30 litres of air; and with a number of others the proportion of air breathed for a given weight of carbonic acid expired also varied, showing that different individuals breathe different volumes of air to supply their body with the necessary amount of oxygen to make and expire a given weight of carbonic acid. It cannot be doubted that the less the volume of air inspired to burn a certain weight of carbon, the more readily the oxygen taken into the lungs finds its way into the blood, and, therefore, the more perfect the respiratory function. This may have important bearings in medical respects. The age of sixty years apparently necessitated breathing a comparatively large proportion of air (11·30 litres) to supply the blood with the oxygen it required. One of the two young men was physically stronger and possessed of a greater muscular development than the other, he breathed 9·29 litres against 10·51 for the other, or took 11·6 per cent. less air into his lungs to yield the necessary oxygen to burn the same weight of carbon within a given time. The corresponding difference between the person aged sixty, and the strongest of the two young men amounted to no less than 17·8 per cent.

X. "On a Pure Fermentation of Mannite and Glycerin." By PERCY F. FRANKLAND, Ph.D., B.Sc. (Lond.), Assoc. Roy. Sch. of Mines, Professor of Chemistry in University College, Dundee, and JOSEPH J. FOX. Communicated by Professor T. E. THORPE, F.R.S. Received June 17, 1889.

Although the fermentative action of micro-organisms has from time to time attracted the attention of numerous investigators, both chemical and biological, still in by far the majority of cases there has been absolutely no guarantee that the chemical changes observed were the result of the activity of a pure growth of one organism and not of a more or less complex mixture of organisms. Indeed, it is only within recent years that the most familiar of all fermentations—the alcoholic—has been induced with growths of yeast definitely ascertained to be of absolute purity.

Thus whilst Pasteur and others had many years previously studied the fermentation of sugar induced by yeast, free from bacteria and other micro-organisms, it is to Hansen that we owe the systematic investigation of the fermentations caused by distinct kinds of yeast in a state of unquestionable purity.

The greatly improved methods of isolating and studying micro-organisms, which we now have at our command, necessitate that all experiments on the chemical changes induced by micro-organisms, should in future be carried out with cultivations of guaranteed purity, *as only under such conditions can the particular reactions be referred to the agency of particular organisms*. It is, moreover, essential that the micro-organisms themselves should be so fully described and characterised as to render possible their identification by other investigators, a point which has been but little attended to in the past. Thus the value of the classical work of the late Albert Fitz on schizomycetic fermentations is not a little diminished by the doubt which attaches to the purity of his cultivation, and to the inadequate description of the micro-organisms he had under observation. On the other hand, of course, these objections in no way detract from the importance of Fitz's work in demonstrating that particular chemical changes can be effected by the agency of bacterial life.

The present paper deals with some of the chemical changes produced by a micro-organism which was obtained in a state of perfect purity by one of us from sheep-dung, which was found to have the power of setting up fermentation in suitable solutions of several carbohydrates and polyhydric alcohols.

Isolation and Morphological Characters of the Fermenting Organism (by Grace C. Frankland and Percy F. Frankland).

A minute quantity of sheep-dung was introduced into test-tubes containing a sterile solution of glucose (3 per cent.) and the necessary mineral ingredients, together with a small quantity of peptone. On placing these tubes in the incubator at 39° C. they were found to be in a state of fermentation on the following day. A minute quantity of this fermenting liquid was transferred by means of a sterile platinum needle into other tubes containing sterile glucose-solution, and in these a similar fermentation was established. A number of further generations were produced by successive transferences in the same way, with the result that in each case a vigorous fermentation was set up.

Some of the fermenting liquid from one of these tubes was then submitted to plate-cultivation with gelatine-peptone in the ordinary way. On the appearance of centres of growth on these plates, inoculations were made from a number of the colonies into tubes containing glucose-solution. On subsequently incubating these tubes some entered into vigorous fermentation, and from one of these a number of inoculations were made into tubes containing glucose, mannite, glycerin, and other solutions to which reference will be made later. The inoculated glucose, mannite, and glycerin tubes all entered into fermentation on being placed in the incubator.

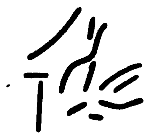
Plates were again poured from a number of these fermenting solutions of various ages, the resulting colonies were carefully examined and again inoculated into the fermentable liquids, and it was only when the purity and uniform character of the organism had thus been fully established that the larger quantities of fermentable material to be chemically examined afterwards were finally inoculated as described below.

Microscopic Appearances.—Under high powers ($\times$ 1000 $\times$ 1500) the organism is seen to be a bacillus with rounded ends, occurring chiefly in pairs, the individual bacilli vary in length from 1·5 μ to 5·1 μ, and in breadth from 0·8 μ to 1·0 μ. Their appearance, however, as is so frequently the case, varies considerably, according to the medium from which they are taken. Thus in the fermenting liquids they were often found to form long threads, whilst in gelatine and other solid media they were usually found in pairs only.

The accompanying figures, Nos. 1 and 2, illustrate the differences in the appearance of the bacilli when taken from gelatine and mannite-solution respectively.

FIG. 1.

FIG. 2.



That this difference in appearance was not accidental but a constant character of the organism was demonstrated by inoculating gelatine-tubes from single colonies of the organism; on microscopically examining the growths in these tubes, the characteristic short bacilli were found, but no threads; on inoculating, however, a series of glucose-tubes from these gelatine cultivations, the long threads were invariably found.

Viewed in drop-cultivations, the bacilli were seen to be extremely motile.

Appearance in Gelatine-Tubes.—The growth is but little characteristic; in the depth the needle-track assumes a beaded appearance, whilst the smooth surface-growth causes more or less rapid liquefaction of the gelatine, according to the temperature and according to the vitality of the organism; thus at low temperatures or when the organism has been inoculated from an old and exhausted previous growth, the liquefaction is very slow.

Appearance in Agar-Agar.—The organism forms an extremely thin and almost invisible growth over the surface, which becomes hardly more conspicuous even when incubated for some time.

Appearance on Potatoes.—Forms a dirty-white, shining growth which extends over almost the whole surface of the potato.

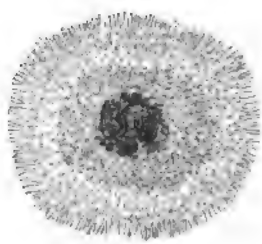
Appearance in Gelatine-Plate Cultivations.—To the naked eye the colonies are very insignificant, appearing as small white dots; later on when liquefaction commences, clear liquid circles form round the small centres, the circles gradually increase in diameter until the whole plate may become involved.

Under a low power ($\times$ 100) the depth-colonies in which neither softening nor liquefaction of the gelatine has commenced, are seen to be smooth-rimmed disks with finely granular contents (see fig. 3), whilst those around which liquefaction has taken place exhibit a dark central mass surrounded by very finely granular matter, the periphery having a delicate hair-like appearance (see fig. 4). The same differences in the rate of liquefaction are observable as in the case of the gelatine-tube cultivations referred to above.

In none of the cultivations were any spores discoverable.

Fig. 3.

Fig. 4.



Fermentation of Mannitol.

The fermentations were carried out in flasks of about 2½ litres capacity; the mannitol was employed in a 3 per cent. solution, 2000 c.c. of solution being placed in each flask. The solution was prepared as follows:—

60 grams of pure mannitol, 2 grams of dry peptone, and 30 grams of precipitated calcium carbonate were placed in the large flask; 200 c.c. of a salt solution* containing the necessary mineral ingredients for the growth of micro-organisms, were diluted to 2000 c.c. with distilled water and then added to the flask containing the mannitol, &c. The flask was then furnished with a plug of sterile cotton-wool, and the whole steam-sterilised for upwards of one hour on three to four successive days.

* This salt solution contained—

Potassium phosphate	5·00 grams	dissolved in 5000 c.c. of water.
Magnesium sulphate (cryst.)	1·00 „	
Calcium chloride (fused)	0·50 „	

The sterile liquid thus obtained was then carefully inoculated with a minute quantity of a pure culture of the organism and placed in an incubator, the temperature of which was maintained at 38—40° C.

The fermentation commences in the course of a few days and continues for several weeks.

Three separate fermentations of mannitol were thus carried on.

In each case, before commencing the chemical examination of the products, the contents of the flask were submitted to microscopic examination and to plate-cultivation with gelatine-peptone; in each case the growth was found to have remained pure, only the characteristic colonies of the organism itself making their appearance on the plates.

Fermentation of Mannitol No. 1.

Two litres of mannitol solution, prepared as above described, were duly sterilised and then carefully inoculated with a minute trace of a pure cultivation of the organism. The liquid was then placed in an incubator the temperature of which was maintained at 38—40° C.

The fermentation commenced on the third day and continued with more or less activity for about one month, but the liquid remained in the incubator for upwards of three months; at the close of this period it was submitted to plate-cultivation as above indicated, and the chemical examination then proceeded with.

Separation of Alcohols.—The fermented liquid, which still contained a quantity of undecomposed carbonate of lime, was distilled down to about one-third of its bulk and until the distillate gave only the faintest indications with Lieben's highly sensitive iodoform reaction for alcohol. The residue in the distilling flask was set aside to be examined for acids (see below), whilst the distillate, which would contain any alcohols, was redistilled over and over again so as to get rid of the greater part of the water. When the volume of liquid amounted to only about 50 c.c. a careful determination of the specific gravity was made, and this indicated the presence of 11·415 grams of absolute alcohol. Finally the liquid was dehydrated with fused carbonate of potash, and on then distilling it passed over at 79—80° C., showing it to be pure ethyl alcohol.

Separation of Volatile Acids.—The residue remaining in the flask, after distilling off the alcohol as above, would contain any acids in the condition of calcium salts; in order to liberate these acids a calculated quantity of hydrochloric acid was added, sufficient exactly to decompose the carbonate of lime originally added to the liquid. On then repeatedly distilling with water the volatile acids passed over and were converted into barium salts for analysis.

The hydrochloric acid was not, however, added all at once, but in several distinct portions, and after each addition the liquid was

severally treated with an excess of pure barium carbonate, boiled for one hour to expel carbonic anhydride, and then filtered; the filtrate containing the barium salt of the volatile acid was then evaporated and weighed after drying until constant at 130° C. A barium determination was then made by conversion into sulphate, and from the proportion of barium sulphate obtained the composition of the volatile acid was calculated. The following results were obtained when the several distillates were treated as above:—

I. 6·636 grams Ba-salt dried at 130° C. gave 91·23 per cent. $BaSO_4$.
II. 4·158 grams Ba-salt gave 91·29 per cent. $BaSO_4$.
III. 2·561 grams Ba-salt gave 91·40 per cent. $BaSO_4$.
IV. 1·217 grams Ba-salt gave 91·83 per cent. $BaSO_4$.
V. 0·321 gram Ba-salt. Barium-determination lost.

With the exception of that in fraction IV, all the barium determinations show that the only volatile acid present was acetic acid, inasmuch as the theoretical quantity of barium sulphate obtainable from barium acetate is 91·37 per cent.

Thus even the proportion of barium sulphate obtained in fraction IV differs but slightly from the theoretical amount for barium acetate, and the discrepancy was probably due to the presence of a little barium carbonate (indicated by slight effervescence on treating the residue with hydrochloric acid, and due to the permanent solubility of the barium carbonate even on boiling), for no formic acid could be discovered by the silver nitrate reaction.

The total amount of Ba-salt obtained, viz., 14·893 grams, may be calculated as pure acetic acid, of which there were, therefore, 7·008 grams produced in this fermentation.

Separation of Fixed Acids, &c.—After distilling off the volatile acid as above, the calcium in solution was precipitated with sodium carbonate, and the solution after filtering was evaporated to dryness. The residue thus obtained was placed in a Soxhlet's apparatus, and extracted with absolute alcohol, which would dissolve any sodium lactate, but leave any sodium succinate, and the greater part of the sodium chloride undissolved. Both the residue and the alcoholic extract, after evaporating the alcohol, were dissolved in water, acidulated with hydrochloric acid, and then repeatedly shaken with ether. The ethereal extract of the solution of the residue yielded on evaporation 0·0215 gram of substance, which on ignition gave the suffocating fumes characteristic of succinic acid.

No lactic acid was found in the ethereal extract of the other portion.

Both the residue and the alcoholic extract referred to above might contain unaltered mannitol, which would have to be separated from

the large quantity of sodium chloride. This separation was partially accomplished by means of strong alcohol in which mannitol is considerably more soluble than sodium chloride; 17·135 grams of mannitol, containing 36·2 per cent. of ash (sodium chloride), were obtained, corresponding to 10·93 grams of pure mannitol.

Fermentation of Mannitol No. 2.

A second fermentation of mannitol was conducted on the same lines as above, the liquid remaining upwards of three months in the incubator at 38—40° C., and then for another month at the ordinary temperature of the air before it was submitted to examination.

Alcohol.—In the alcoholic distillate there was obtained after repeated rectification a liquid of specific gravity indicating the presence of 9·519 grams of absolute alcohol, on further rectification the specific gravity indicated 9·094 grams alcohol, and after dehydration with fused carbonate of potash, 7·75 grams of liquid boiling at 79—80° C. were actually recovered. The amount of ethyl alcohol actually produced in the fermentation may thus be estimated at about 9·3 grams.

Volatile Acids.—From the several fractions of the volatile acids, the following barium salts were prepared:—

I. 6·3435 grams Ba-salt giving 91·61 per cent. $BaSO_4$.
II. 2·9525 grams Ba-salt giving 91·35 per cent. $BaSO_4$.
III. 0·8080 gram Ba-salt giving 93·88 per cent. $BaSO_4$.
IV. 2·0385 grams Ba-salt. (The Ba-determination in this salt yielded 116·2 per cent. of $BaSO_4$, an obviously erroneous figure: unfortunately the determination could not be repeated, the remainder of the salt having been lost.)

Remembering the calculated proportions of barium sulphate for—

Barium acetate........	91·37 per cent.
„ formate........	102·64 „

it is obvious that the fractions I and II consisted of pure barium acetate, fraction III of barium acetate with a small admixture of barium formate, whilst fraction IV, in which unfortunately no satisfactory Ba-determination was obtained, doubtless contained a larger proportion of formate.

The presence of formic acid in fractions III and IV was further demonstrable by means of the reaction with silver nitrate.

The amount of barium acetate and formate respectively present in mixtures of the two salts may be calculated from the following formula—

$$Bx = 100P + bx - 100b,$$

in which

x = percentage of barium formate,
P = percentage of $BaSO_4$ actually found,
B = percentage of $BaSO_4$ theoretically obtainable from Ba-formate,
b = ditto from Ba-acetate.

Thus the calculated composition of

	Barium acetate.	Barium formate.
Fraction III	0·6281	0·1799

The total quantity of barium salts of the volatile acids obtained from the fermented liquid thus amounted to 12·1425 grams; of which probably about 1 gram was barium formate.

Taking the whole of the barium salts as barium acetate, the quantity of volatile acid produced in this fermentation amounted to 5·7141 grams calculated as acetic acid.

Fixed Acids, &c.—The mode of dealing with the residue left after distilling off the volatile acids was somewhat modified in this case. The calcium was precipitated with sodium carbonate and filtered off; after which the filtrate was acidulated with hydrochloric acid, and repeatedly shaken with ether to remove any lactic or succinic acids; from the residue insoluble in ether, a quantity of mannite amounting to 15·716 grams was recovered. The ethereal extract was evaporated, neutralised with sodium carbonate, again evaporated, and then extracted with strong alcohol (97 per cent.) to dissolve out any sodium lactate, but none was found; on the other hand, the sodium salt insoluble in alcohol on acidulation and extraction with ether yielded 0·034 gram succinic acid.

Thus the results obtained in the second fermentation were essentially the same as those yielded by the first, with the exception that in addition to ethyl alcohol and acetic acid a small proportion of formic acid was also produced, this is possibly to be accounted for as the result of a secondary action of the ferment on the acetic acid produced in the first instance. This conversion of acetic into formic acid being a process of oxidation, may be dependent upon the accession of air to the liquid at the close of the main fermentation.

Fermentation of Mannite No. 3.

In this case the fermenting liquid was allowed to remain in the incubator at 38—40° C. for three months, and then at the ordinary temperature of the air for nearly two months more.

The alcoholic portion was unfortunately lost, but the volatile acids yielded the following barium salts :—

I. 7·623 grams Ba-salt giving 93·76 per cent. $BaSO_4$.
II. 2·485 grams Ba-salt giving 96·39 per cent. $BaSO_4$.

Both of these fractions were, therefore, mixtures of barium acetate and formate, and by applying the formula given above their composition is found to be—

	Barium acetate.	Barium formate.
I........	6·0062 grams	1·6168 grams.
II........	1·3782 „	1·1068 „
	7·3844 „	2·7236 „

The total quantity of barium acetate obtained thus amounted to 7·3844 grams, corresponding to 3·4750 grams of acetic acid, whilst the barium formate amounting to 2·7236 grams corresponds to 1·1038 grams of formic acid.

In the residue, after distilling off the volatile acids, 0·027 gram of succinic acid was obtained.

Fermentation of Glycerol.

Pure cultivations of the same organism on being introduced into perfectly similar solutions containing glycerol instead of mannitol caused a vigorous fermentation of the former. The fermentation was, however, distinctly less energetic than in the case of the mannitol, for it was later in commencing and continued over a longer period of time. The investigation of the products of this fermentation of glycerol was carried out on precisely the same lines as in the case of the mannitol. In each case 60 grams of pure glycerol were submitted to fermentation in 2 litres of water, to which the same additions were made as in the case of the mannitol referred to above. The liquid was fermented at 37—39° C., and before commencing the chemical examination of the products it was submitted to plate cultivation, with the result that no foreign organisms were discoverable.

Fermentation of Glycerol No. 1.

This fermentation continued over a period of three months, after which the chemical examination of the products was immediately proceeded with.

Alcohols.—After repeated distillation a liquid was obtained weighing 63·58 grams, and of specific gravity 0·9871, corresponding to 4·90 grams of absolute alcohol. Unfortunately, owing to an accident, the liquid was lost and the alcohol was not further isolated.

Volatile Acids.—Proceeding as in the case of the mannitol fermentations, the following barium salts were obtained :—

I.	2·8505 grams Ba-salts giving	91·66 per cent. $BaSO_4$.		
II.	3·2630 „ „	93·17 „ „		
III.	0·5360 gram Ba-salts.			
	6·6495			

Pure Ba-acetate yields 91·37 per cent. $BaSO_4$.
Pure Ba-formate yields 102·64 per cent. $BaSO_4$.

Thus, whilst the first fraction was practically pure barium acetate, the second fraction contained a notable proportion of barium formate, the composition of this fraction, as calculated by the aid of the formula given on p. 351, being—

Barium acetate	2·7419 grams
„ formate	0·5211 „
	3·2630 „

The third fraction doubtless contained an even larger proportion of barium formate, but the barium determination was unfortunately lost.

It may, however, be taken that the volatile acids produced in this fermentation consisted of about 2·75 grams acetic and 0·32 gram formic acid.

Fixed Acids.—After distilling off the volatile acids as above, the residue containing any fixed acids, together with calcium chloride and unaltered glycerol, was treated with sodium carbonate to precipitate the calcium, which was filtered off; the filtrate was evaporated to dryness and extracted with a mixture consisting of 5 vols. of alcohol and 1 vol. of ether. Such a mixture, whilst dissolving glycerol, does not dissolve either sodium lactate or succinate. From the ether-alcohol extract 22·8615 grams of unaltered glycerol* were recovered. The residue, insoluble in ether-alcohol, was treated with strong boiling alcohol to extract any sodium lactate, but none was obtained, the residue remaining after treatment with alcohol was dissolved in water, acidulated with hydrochloric acid, and repeatedly shaken up with ether; from this ethereal extract 0·079 gram of succinic acid was obtained.

The results show that only a part of the glycerol had undergone fermentation (nearly one half having been recovered), whilst the principal products were alcohol and acetic acid, together with a small quantity of formic acid and traces of succinic acid.

Fermentation of Glycerol No. 2.

In this experiment the fermenting liquid was allowed to remain in the incubator at 37—39° C. for five months before examination.

* This glycerol yielded on incineration only 0·85 per cent. of ash.

Alcohol.—Repeated distillation yielded 63·4775 grams of liquid, of specific gravity 0·9803, corresponding to 8·20 grams of absolute alcohol; after further rectification the weight of liquid was reduced to 54·0 grams, of specific gravity 0·9776, corresponding to 8·14 grams absolute alcohol. By dehydrating this with fused potassium carbonate, 7·518 grams of alcohol, boiling at 79° C., were obtained.

Volatile Acids.—Proceeding as before, the following amount of barium salt was obtained:—

Total barium salt = 8·239 grams, giving 91·08 per cent. $BaSO_4$.
Pure Ba-acetate gives 91·37 per cent. $BaSO_4$.

Thus the whole of the salt was practically pure barium acetate, although a trace of formic acid was discoverable qualitatively.

The total quantity of acetic acid, as calculated from the barium salt, amounted to 3·877 grams.

Fixed Acids.—Using the same method of separation as described in the first fermentation of glycerol, 0·063 gram of succinic acid was found, whilst 24·19 grams of unaltered glycerol were recovered. This glycerol on incineration yielded only 0·96 per cent. of ash.

The results obtained in this fermentation, therefore, fully substantiate those of the first. The glycerol was only very partially decomposed, the principal products being again alcohol and acetic acid, with traces of formic and succinic acids.

In the fermentation of the glycerol, as in that of the mannitol, it would appear that the formic acid is produced at the expense of the acetic acid, for in this second fermentation of glycerol, in which only traces of formic acid were discovered, the proportion of acetic acid was considerably greater than in the first fermentation, in which a notable quantity of formic acid was found.

The results obtained in the several fermentations may be tabulated as follows:—

Mannitol Fermentations.

	Alcohol.	Acetic acid.	Formic acid.	Succinic acid.
I..	11·415 grams	7·008 grams	..	0·0215 gram
II..	9·3 "	[5·7141*] "	..	0·034 "
III..	..	3·475 "	1·104 grams	0·027 "

Glycerol Fermentations.

	Alcohol.	Acetic acid.	Formic acid.	Succinic acid.
I..	[4·90] grams	[2·75] grams	[0·32 gram]	0·079 gram
II..	8·17 "	3·877 "	trace	0·063 "

* Total volatile acid calculated as acetic acid.

Remarks.

(1.) Both mannitol and glycerol are fermented by this organism, with production of essentially the same substances, viz., ethyl alcohol and acetic acid, together with smaller quantities of formic and succinic acids.

The proportion of formic acid in both fermentations appears to be formed at the expense of the acetic acid, inasmuch as in all cases the amount of acetic acid found varied inversely as the proportion of formic acid. The proportion of succinic acid, although in both cases very small, was distinctly greater in the glycerol than in the mannitol fermentations.

(2.) The proportion of alcohol to acetic acid in the mannitol fermentations is constant, viz., as 1·63 : 1, which corresponds to the molecular proportions.

$$\underset{(2\times 46)}{2C_2H_5{\cdot}OH} : \underset{(60)}{CH_3{\cdot}COOH} = 1{\cdot}53$$

(3.) Excluding the first glycerol fermentation, in which the alcohol found was doubtless too small, the proportion of alcohol to acetic acid is as 2·11 : 1, corresponding to the molecular proportions,

$$\underset{(3\times 46)}{3C_2H_5{\cdot}OH} : \underset{(60)}{CH_3{\cdot}COOH} = 2{\cdot}30$$

(4.) In all the fermentations the decomposition was only an incomplete one, a considerable part of the mannitol, and especially of the glycerol, being in each case recoverable after the fermentation was finished. We propose subsequently to investigate the cause of this limitation.

(5.) In the mannitol fermentation it is impossible to determine whether the succinic acid is formed by a process of synthesis or analysis, but in the fermentation of glycerol it obviously has a synthetical origin.

(6.) In addition to ethyl alcohol, there appears to be also a small proportion of some higher alcohol produced, for in the alcoholic distillation the distillate was at first somewhat turbid, but became clear again when a larger quantity had collected.

(7.) We have introduced pure cultivations of the same bacillus into solutions of a number of different substances likely to undergo fermentation. The bacillus, as already mentioned, ferments glucose vigorously, it also more slowly ferments cane-sugar, milk-sugar, starch, and calcium glycerate; the products of these fermentations are being further investigated by one of us. On the other hand, we have been unable to cause it to ferment solutions of dulcite, erythrite, ethylene glycol, calcium lactate, tartrate, citrate, or glycollate.

Its inactivity towards dulcite is particularly interesting, and furnishes another instance of the selective power of micro-organisms towards the most closely allied isomeric bodies. Remembering the relationship of dulcite to galactose and of galactose to milk-sugar (galactose is converted into dulcite by nascent hydrogen, and milk-sugar is converted into galactose and dextrose by the action of dilute acids), it is to be anticipated that in the fermentation which this bacillus induces in milk-sugar, the decomposition is limited to the dextrose portion of the milk-sugar molecule. In the action of this bacillus on starch, the latter is in the first instance dissolved, doubtless through the agency of a diastatic ferment, to which the organism gives rise, as well as to the peptonising one which brings about the liquefaction of the gelatine already referred to. A tube containing starch-liquid, which had been fermented by the bacillus, gave no blue coloration with iodine, clearly showing that the whole of the starch had undergone transformation into other products.

(8.) In view of the characteristic products—ethyl alcohol and acetic acid—to which this organism gives rise, we propose for it the name of *Bacillus ethaceticus*.

XI. "On the Effect of Temperature on the Specific Inductive Capacity of a Dielectric." By W. CASSIE, M.A. Communicated by Professor J. J. THOMSON, F.R.S. Received May 24, 1889.

(Abstract.)

The variation with temperature of specific inductive capacity was measured in different ways for solids and liquids.

In the case of solids a condenser was made with thin sheets of the dielectric in question, and the capacity measured at different temperatures. The condenser was suspended in an air-bath by wires passing through the top to an insulated support outside. This support was several feet above the bath, so that it was never heated, and its insulation was independent of the temperature of the condenser. The capacity was measured by Professor J. J. Thomson's method,* and conduction or absorption in the condenser allowed for by varying the time of charge and discharge. The rate of increase per degree centigrade of the specific inductive capacity was found to be for—

	Mica	between	11°	and	110°	.... 0·0003
	Ebonite	„	13	„	63	.... 0·0004
	Glass	„	17	„	60	.... 0·0012
Another specimen of	„	„	13	„	60	.... 0·002

* 'Phil. Trans.,' 1883.

In the case of liquids a quadrant electrometer was immersed in the liquid in question, and the deflection observed at different temperatures. The liquid was heated in a water-bath, and the needle and quadrants were attached to insulating supports above the bath. The electromotive force was obtained from a Ruhmkorff coil without the condenser, and with a high resistance between the terminals by which to control the E.M.F. The poles of a second electrometer in air were connected to the poles of the liquid electrometer, and the ratio of the readings of these two gave a measure of the specific inductive capacity independent of variations of E.M.F. The results are shown in the following table; and in the last column are inserted for comparison the rate of change of refractive index for the four of the liquids for which Messrs. Dale and Gladstone have determined it. Mean values are given except for these four. For glycerine there is no similarity between the two effects; but for the other three the effects are of the same order of magnitude, although not exactly in the ratio 1 : 2 indicated by the electromagnetic theory of light.

	Rate of decrease of specific inductive capacity per degree.		Rate of decrease of refractive index per degree for A line in solar spectrum.	
Turpentine.......	between 20° and 36°	0·0012	between 10° and 47°	0·00035
"	" " 49	0·0011		
"	" " 62	0·0009		
Carbon bisulphide.	" 15 and 43	0·004		
Glycerine	" 18 and 41	0·006	between 20° and 48°	0·00018
"	" " 61	0·0053		
Benzoline	" 19 and 41	0·0006	between 25° and 39°	0·00037
"	" " 52	0·0011		
"	" " 63	0·0015		
Benzine	" 15 and 39	0·0014	between 10° and 39°	0·0004
"	" " 58·5	0·0012		
Olive oil	" 17 and 68	0·0024		
Paraffin oil.......	" 18 and 54	increase 0·0023		

XII. "On the Interchange of the Variables in certain Linear Differential Operators." By E. B. ELLIOTT, M.A., Fellow of Queen's College, Oxford. Communicated by Professor SYLVESTER, F.R.S. Received June 5, 1889.

(Abstract.)

Recent theories of functional differential invariants, reciprocants, cyclicants, &c., have brought into notice a considerable number of

linear differential operators, whose arguments are the derivatives of one with regard to the others of a set of variables connected by any single relation. By aid of such operators, in their quality of annihilators or generators, the forms of classes of functions of the derivatives having properties of persistence in form after various classes of transformations have been discussed with some completeness, and great light has been thrown on the properties of other functions in connexion with such transformations. Often, however, in cases where the transformations dealt with have not been symmetrical in all the variables, the investigation has presupposed that a certain one, or one of a restricted set, of the variables has been chosen as the dependent variable. A complete theory of the interchange of the variables in the classes of functions has been a desideratum, and towards the attainment of that end a theory of the interchange of the variables in the operators has been a first requirement. Such a theory it is the aim of the present memoir to supply for the cases of two and of three variables. I speak of the operators appertaining to the two classes of cases as binary and ternary operators respectively. For the binary operators dealt with I adopt a general form, which is a slight extension of one introduced in an able investigation of Major MacMahon's, and for the ternary operators one that is closely analogous.

I. *Binary Operators.*

By x and y are denoted two variables connected by any relation. By x_r and y_r are meant $\dfrac{1}{r\,!}\dfrac{d^r x}{dy^r}$ and $\dfrac{1}{r\,!}\dfrac{d^r y}{dx^r}$ respectively. Let ξ and η be corresponding finite increments of x and y, so that

$$\xi = x_1\eta + x_2\eta^2 + x_3\eta^3 + \ldots\ldots,$$

and consequently

$$\begin{aligned}\xi^m &= (x_1\eta + x_2\eta^2 + x_3\eta^3 + \ldots\ldots)^m \\ &= X_m^{(m)}\eta^m + X_{m+1}^{(m)}\eta^{m+1} + X_{m+2}^{(m)}\eta^{m+2} + \ldots\ldots, \text{ say.}\end{aligned}$$

In like manner let $Y_m^{(m)}$, $Y_{m+1}^{(m)}$, $Y_{m+2}^{(m)}$, ... be defined.

Denote the operator

$$\frac{1}{m}\Sigma\left\{(\mu+\nu s)X_s^{(m)}\frac{d}{dx_{n+s}}\right\} \text{ by } \{\mu,\, \nu\,;\ m,\, n\}_x,$$

the summation being with regard to s, which assumes in turn all integral values not less than the least of m and $-n+1$. Fractional values of m and n are not admissible, but their integral values may

be either positive or negative. The value zero of n is admitted, and that of m, though somewhat special, is not excluded.

What is sought and effected is the expression of any such operator $\{\mu, \nu; m, n\}_x$ in terms of operators of the same form $\{\mu', \nu'; m', n'\}_y$, in y dependent. The process depends on the use of a certain symbolical form for $\{\mu, \nu; m, n\}_x$, and on the proof that a simple factor produces from that symbolical form the symbolical form of the equivalent y operator.

If $m+n \geqq 1$, so that none of the coefficients of powers of η in ξ^m is wanting from $\{\mu, \nu; m, n\}_x$, the inclusive formula of transformation is found to be—

$$\{\mu, \nu; m, n\}_x = -\left\{\nu(n+1), \frac{\mu}{m}; n+1, m-1\right\}_y;$$

and the conclusion is deduced, among others, that there are two classes of self reciprocal operators, a class of positive and one of negative character, viz.:—

$$\{-m, 1; m, m-1\}_x = \{-m, 1; m, m-1\}_y,$$

and
$$\{m, 1; m, m-1\}_x = -\{m, 1; m, m-1\}_y.$$

Particular attention is devoted to the special cases of $m = 0$ and $n = -1$; also to the transformation of Ω and V, the annihilators of invariants and of pure reciprocants. V of course, not involving the first derivative, is not an operator of the class $\{\mu, \nu; m, n\}$ itself, but is linear in such operators.

If $m = -n$ the formula of transformation is found to be

$$m\{\mu, \nu; m, -m\}_x$$
$$= -\{\nu m(1-m), \mu; 1-m, m-1\}_y + (\mu+m\nu)y_1^{-m}\{0, 1; 1, -1\}_y.$$

In particular

$$m\left\{\mu, -\frac{\mu}{m}; m, -m\right\}_x = (1-m)\left\{\mu, -\frac{\mu}{1-m}; 1-m, m-1\right\}_y.$$

If
$$m+n<0, = -r \text{ say, it is}$$

$$\begin{aligned} m\{\mu, \nu; m, -m-r\}_x = & -\{\nu m(1-m-r), \mu; 1-m-r, m-1\}_y \\ & +(\mu+\nu m)X_m^{(m)}\{0, 1; 1-r, -1\}_y \\ & +(\mu+\nu m+\nu)X_{m+1}^{(m)}\{0, 1; 2-r, -1\} \\ & + \ldots\ldots \\ & +(\mu+\nu m+\nu r-\nu)X_{m+r-1}^{(m)}\{0, 1; 0, -1\} \\ & +(\mu+\nu m+\nu r)X_{m+r}^{(m)}\{0, 1; 1, -1\}_y. \end{aligned}$$

II. *Ternary Operators.*

Let x, y, z be variables connected by a relation of any form known or unknown. Let x_{rs}, y_{rs}, z_{rs} denote respectively

$$\frac{1}{r\,!\,s\,!}\,\frac{d^{r+s}x}{dy^r dz^s},\quad \frac{1}{r\,!\,s\,!}\,\frac{d^{r+s}y}{dz^r dx^s},\quad \frac{1}{r\,!\,s\,!}\,\frac{d^{r+s}z}{dx^r dy^s}.$$

Let $$\xi^m = \left\{\sum_{p+q<1} x_{pq}\eta^p\zeta^q\right\}^m = \sum_{r+s<m} X_{rs}^{(m)}\eta^r\zeta^s$$

be the expansion of the mth power of an increment of x in terms of the corresponding increments of y and z; and define as

$$m\{\mu,\ \nu,\ \nu'\,;\ m,\ n,\ n'\}_x$$

the operator $$\Sigma(\mu+\nu r+\nu' s)X_{rs}^{(m)}\,\frac{d}{dx_{n+r,\,n'+s}},$$

and as $m\{\mu,\ \nu,\ \nu'\,;\ m,\ n,\ n'\}_y$, $m\{\mu,\ \nu,\ \nu'\,;\ m,\ n,\ n'\}_z$, the operators obtained from this by cyclically interchanging x, y, z once and twice respectively.

Attention is confined to positive integral values of m, except that the value zero of m is admitted, in so far as its admission requires the introduction of no new idea. By n and n' are denoted positive integers or zeroes, or in certain special cases -1. Thus the field of investigation is narrower than the analogue of that covered in dealing with binary operators.

The comprehensive theorem for the transformation of these ternary operators is that

$$\{\mu,\ \nu,\ \nu'\,;\ m,\ n,\ n'\}_x = \left\{-\nu(n+1),\ \nu',\ -\frac{\mu}{m}\,;\ n+1,\ n',\ m-1\right\}_y$$
$$= \left\{-\nu'(n'+1),\ -\frac{\mu}{m},\ \nu\,;\ n'+1,\ m-1,\ n\right\}_z.$$

There are three classes of cyclically persistent operators, of different characters, each corresponding to a cube root of unity, viz. :—

$$\{-m,\ 1,\ 1\,;\ m,\ m-1,\ m-1\}_x = \{-m,\ 1,\ 1\,;\ m,\ m-1,\ m-1\}_y$$
$$= \{-m,\ 1,\ 1\,;\ m,\ m-1,\ m-1\}_z,$$

$$\{-m,\ \omega,\ \omega^2\,;\ m,\ m-1,\ m-1\}_x = \omega\{-m,\ \omega,\ \omega^2\,;\ m,\ m-1,\ m-1\}_y$$
$$= \omega^2\{-m,\ \omega,\ \omega^2\,;\ m,\ m-1,\ m-1\}_z,$$

$$\{-m,\ \omega^2,\ \omega\,;\ m,\ m-1,\ m-1\}_x = \omega^2\{-m,\ \omega^2,\ \omega\,;\ m,\ m-1,\ m-1\}_y$$
$$= \omega\{-m,\ \omega^2,\ \omega\,;\ m,\ m-1,\ m-1\}_z.$$

Most of the ternary operators which in recent investigations have had their importance established, do not involve first derivatives. They are the results of replacing first derivatives by zeroes in operators such as above, or may be regarded as linear functions of different operators. The transformation of the various annihilators of pure and projective cyclicants is considered from the latter point of view.

It is indicated, however, without much development that, if preferred, it is possible to consider the transformation of operators free from first derivatives without use of operators in which those derivatives occur. In illustration of the method it is established that, if $[\mu, \nu, \nu'; m, n, n']$ denote that part of $\{\mu, \nu, \nu'; m, n, n'\}$ which is free from first derivatives,

$$(x_{10}x_{01})^{\frac{1}{2}(1-m)}[-m, 1, 1;\ m, 0, 0]_x = (y_{10}y_{01})^{\frac{1}{2}(1-m)}[-m, 1, 1;\ m, 0, 0]_y$$
$$= (z_{10}z_{01})^{\frac{1}{2}(1-m)}[-m, 1, 1;\ m, 0, 0]_z$$

gives for different values of m a class of cyclically persistent operators.

XIII. "On the Rate of Decomposition of Chlorine-water by Light." By G. Gore, LL.D., F.R.S. Received June 13, 1889.

(Abstract.)

In this research, the author has investigated by means of the voltaic balance the kind and amount of chemical change, the rate at which decomposition proceeds, and the chemical composition of the products formed at all stages of decomposition of chlorine-water, when exposed to daylight and sunlight in colourless glass vessels.

The chlorine-water, by exposure to diffused daylight, was decomposed with moderate uniformity, but at a gradually diminishing rate, as shown by the losses of voltaic energy, until no further loss of such energy occurred; the liquid then consisted of an aqueous solution of hydrochloric acid, hypochlorous acid, and chloric acid. By further exposure of the liquid to daylight and sunlight during several weeks, peroxide of hydrogen was formed; and the amount of hydrochloric acid and of voltaic energy very slowly increased until that of the latter became about equal to that of dilute hydrochloric acid of equivalent strength to the whole of the chlorine present; all the other chief properties of the final liquid agreed with those of a mixture of dilute hydrochloric acid and peroxide of hydrogen. Still further exposure to strong sunlight caused no further change in chemical composition, amount of voltaic energy, or other property of the liquid.

This research shows distinctly that the decomposition of chlorine-water by light may be divided into two essentially different parts or periods of chemical change, and that the kinds of chemical change occurring during these two periods are largely different. During the first period, a very great and gradual *loss* of voltaic energy occurs, attended by formation of hydrochloric, hypochlorous, and chloric acids. During the second period, a moderate and very slow *increase* of voltaic energy takes place, accompanied by decomposition of the hypochlorous and chloric acids, a further formation of hydrochloric acid, and the production of peroxide of hydrogen. Under the influence of prolonged sunlight, the whole of the oxygen of the hypochlorous and chloric acids united with water to form peroxide of hydrogen, and the peroxide then combined with the whole of the hydrochloric acid to form a definite "solution compound" represented by the formula $2HCl,H^2O^2$.

The chemical composition of the products of the change at the ends of the first and second periods of change was ascertained by means of the voltaic balance and ordinary chemical analysis. During the first period forty consecutive measurements of the voltaic energy at stated intervals of time were made, and the energy diminished from about 1219 millions to 2·9 millions; and during the second period eight such measurements were made, and the energy increased to 9·3 millions. A curve is given showing the rate of loss of energy during the first period.

It is interesting to observe, that suitably decomposed chlorine-water, or, in its stead, a mixture of $6HCl + HClO + HClO^3$ dissolved in a proper proportion of water, has the property of absorbing energy by exposure to light, very much like that possessed by the green leaves of plants.

XIV. "Barium Sulphate as a Cement in Sandstone." By FRANK CLOWES, D.Sc., Principal and Professor of Chemistry and Metallurgy in University College, Nottingham. Communicated by Professor ARMSTRONG, F.R.S. Received June 6, 1889.

About six miles west of Nottingham there are two prominent conical sandstone hills which are so different in appearance from any surrounding elevations of the surface as to arrest attention. These are known as Stapleford Hill and Bramcote Hill. In the gap between them stands a remarkable sandstone pillar, some 30 feet in height, and 70 feet in circumference, and distinctly crowned by a flattened mushroom-shaped cap. This is locally known as the Hemlock Stone. The sandstone beds forming these hills and columns have been classed

by geologists as the Keuper basement beds of the Trias. But no satisfactory explanation has yet been given of the power possessed by these bold elevations to resist the denudation which has removed the surrounding deposit.

An analysis of a sample of sandstone from the top of the Hemlock Stone was made a few years since by R. F. Blake and A. P. Beddard, then students in my laboratory. They detected a large amount of barium sulphate in a crystalline condition, inclosing and binding together the grains of sand. This appeared to furnish a clue to the cause of the resistent power possessed by the stone. In the company of Professor Blake, I proceeded accordingly to collect specimens of the sandstone present at different levels of the Hemlock Stone and of the two adjacent hills, and to subject them to qualitative analysis.

Qualitative Composition.—The cap of the Hemlock Stone was found to be free from carbonates and from calcium, and contained much barium sulphate; the stone was compact and hard. The soft loose sandstone at certain lower levels, however, contained some calcium sulphate and a variable proportion of calcium carbonate, the barium sulphate being in much smaller quantity than in the top; sodium was also present in fair proportion. The resistent power conferred by the almost insoluble barium sulphate upon the top of the stone would probably explain its projecting cap-like shape, the lower portions having suffered by weathering to a far larger extent, owing to the more soluble nature of the calcium compounds which they contained, and to the smaller proportion of the barium sulphate.

The sandstone of which Bramcote Hill is composed showed a variation in chemical composition similar to that of the Hemlock Stone. The sandstone at the summit of the hill was free from carbonates, and contained much barium sulphate; this was the general character of the stone at lower levels, but one sample, taken about half-way up the hill, contained calcium carbonate and a less proportion of barium sulphate.

Stapleford Hill, on the other hand, showed throughout an absence of carbonates and of calcium, while barium sulphate was always present, and usually in quantity.

Distribution of the Barium Sulphate.

In certain portions of the sandstone the barium sulphate is evenly distributed throughout the mass of the rock, giving on fracture a surface of compact and uniform appearance. In other portions the sulphate is seen on the surface produced by fracture as a network of light-coloured compact veins inclosing darker and more or less loose sand grains; the weathered surface in this case presents a honey-

combed or fretted appearance, which is particularly noticeable on the cap of the Hemlock Stone. In other parts, the fractured sandstone shows compact blotches of sand cemented by the sulphate, with intervening loose and darker portions; the stone at the upper part of Bramcote Hill is of this character, and after prolonged weathering it has been converted into what has been called the pebble bed; this is a loose sand bed inclosing rounded pebble-like masses of sand grains bound together by the sulphate; these are usually about the size and shape of a hazel-nut.

Quantitative Composition.—As this sandstone appears to be unique in composition amongst the sandstones of this country, quantitative analyses were made of the portions which contained the largest proportion of barium sulphate. They yielded the following results:—

	Hemlock Stone.		Stapleford Hill.		Bramcote Hill.	
	Top.	Near base.	Top.	Base.	"Pebbles" at top.	Middle height.
	1	2	3	4	5	6
Loss at 100° C. moisture ..	0·21	0·24	0·11	0·20	0·18	0·05
Loss by ignition (organic matter, &c.)	0·87	2·13	0·36	0·39	0·93	0·72
$Fe_2O_3 + Al_2O_3$............	6·41	4·84	3·53	4·46	5·10	4·45
BaO....................	30·23	21·89	30·81	32·80	18·52	33·30
CaO....................	0·00	1·68	0·02	0·00	0·00	0·00
MgO....................	0·13	1·08	0·10	0·00	0·00	0·00
SO_3....................	16·39	12·09	16·58	17·14	10·14	17·42
SiO_2....................	44·46	54·52	47·36	43·77	62·59	41·47
Alkalis, &c. (by difference).	1·30	1·53	1·13	1·24	2·54	2·59
$BaSO_4$ present........	46·03	33·33	46·92	49·95	28·20	50·06

Weathering.—It would be inferred from the composition of this sandstone, that it would suffer loss only with extreme slowness by weathering, owing to the very slight solubility under ordinary conditions of the barium sulphate. Experiments were made to confirm this supposition by treating finely powdered specimens of Nos. 1 and 2, the analyses of which have been given above, with hot dilute hydrochloric acid for some considerable time. Analyses of the solid matter dissolved by the acid gave the following results:—

	No. 1.	No. 2.
Loss at 100°	0·21	0·21
Loss by ignition	0·87	1·60
In solution in HCl :—		
$Fe_2O_3 + Al_2O_3$	0·97	2·34
CaO	0·00	1·01
MgO	0·13	0·22
SiO_2	0·66	1·20
Alkalis, &c. (by difference)	0·85	1·33
Insoluble in HCl	96·31	92·09

It may be assumed that the loss which the sandstone suffers by long treatment in the form of powder with dilute hydrochloric acid, will be greater than that caused by very long weathering, since the hot acid is a more powerful solvent than rain-water, and it is further enabled to attack the whole mass of the stone instead of the surface only. Under these extreme conditions, however, the stone forming the cap of the Hemlock Stone lost only 3·7 per cent. of its weight. Its protective power as an unwasting cover over the column beneath is therefore easily understood. The whole substance of the two hills will evidently behave in a similar way to the top of the Hemlock Stone, and render them practically permanent under the action of weathering.

Microscopic Examination.—Professor Lebour kindly undertook the microscopic examination of a fine section of this sandstone, prepared by Mr. G. Healey. He reports that "the cementing material is undoubtedly crystalline barium sulphate," and that "besides the quartz grains, there are others of much the same average size and shape, the nature of which is not clear." The quartz grains are "more angular than rounded, and include narrow, rod-like crystals, which in all likelihood are apatite."

Occurrence of Barium Sulphate in other Sandstones.

The occurrence of barium sulphate in British sandstones has not been hitherto noticed; this at least was the verdict of the geologists assembled at the British Association meeting at Aberdeen, before whom a preliminary announcement of the discovery was made ('Brit. Assoc. Report,' 1885, p. 1038).

A careful examination of a large number of specimens of sandstone from the neighbourhood of Nottingham, and from other parts of the country, has confirmed this verdict. I have in no case found even a trace of any barium compound. Mr. H. T. Brown, F.R.S., however,

after examining at my request a number of specimens in his possession, found in one of them a small quantity of barium, entirely in the form of carbonate. This specimen was taken from the rock of Beeston Castle Hill, Cheshire, from Keuper basement beds, of the same formation, therefore, as those in which the sulphate has been found in this neighbourhood. In the geology of the neighbourhood of Chester ('Geol. Survey Memoirs,' 1882, pp. 7, 8), Mr. Aubrey Strahan, M.A., states that barium sulphate occurs in cracks in the rock of Beeston Castle: and further infers that the sulphate may exist as a cementing material, since the cement gives a sulphur reaction with the blowpipe. The presence of the sulphate in cracks of the stone is similar to its presence in crystal tufts in the cracks of the septaria of the London clay, which has been long known. The occurrence of the sulphate over a large area in the Nottingham district, as well as in Cheshire, seems to indicate that the substance may be characteristic of the particular formation in which it occurs in both localities, namely the Keuper basement beds.

On the Continent the occurrence of barium sulphate, under conditions more or less similar to those recorded above, has been recorded by Bischof ('Chem. and Phys. Geology,' vol. 1, p. 433). Sandstone cemented by barium sulphate, was found by him in Münzenberg, in the Witterau. He also describes disintegrated granite as being found cemented by the sulphate on the declivities of the Morvan; and sand and clay were found compacted by the sulphate in the district of Keuznach.

Process of Deposition.

There seems little to indicate the way in which the nearly insoluble sulphate has been deposited in the sand in the case under consideration. There is the possibility of barium sulphide in solution becoming insoluble by oxidation, as suggested by Professor Church; or the sulphate itself may exist in solution under conditions at present unknown, and be deposited directly from its solution.

The fact that calcium sulphate was detected in some specimens of the sandstone which I examined, and that barium carbonate was found in similar beds in Cheshire, lends probability, however, to the formation of the sulphate by double decomposition. Bischof (*loc. cit.*) describes experiments in which solid barium carbonate, as well as barium bicarbonate or barium silicate in solution, were readily converted into barium sulphate by the action of any soluble sulphate, such as the sulphate of calcium or of magnesium. And Haidinger (Poggendorff's 'Annalen,' vol. 11, p. 376) has traced the change in progress in nature; barium carbonate present in mountain limestone of Alston Moor being found by him undergoing slow conversion from

the surface inwards into the crystalline sulphate. Further, barium sulphate is known to occur as a pseudomorph of barytocalcite.

The abundant presence of calcium sulphate in the sandstone in the neighbourhood of Nottingham points to the probability of the barium sulphate having been formed *in situ* by the process of double decomposition, and probably from barium carbonate which has been found elsewhere in sandstone beds of the same formation.

XV. "Deposits of Barium Sulphate from Mine-water." By FRANK CLOWES, D.Sc., Principal and Professor of Chemistry and Metallurgy in University College, Nottingham. Communicated by Professor ARMSTRONG, F.R.S. Received June 6, 1889.

Specimens of remarkable deposits which have formed in the water-boxes of coal-mines in the neighbourhood of Newcastle-upon-Tyne have been for many years in the possession of the Museum of the Durham College of Science.

A rough analysis has been published by J. T. Dunn ('Transactions of the Newcastle Chemical Society,' vol. 3, p. 261) of an apparently similar deposit, which was formed during the autumn of 1876 in the Jane Pit at Walker. During this short period the sectional area of the box had been reduced by the deposit from seven and a half square inches to less than half a square inch, and had been in places almost completely closed. The deposit consisted of layers which were alternately white and brown, and was moderately soft. Analysis showed the percentage composition to be 90 of $BaSO_4$, 8 of $SrSO_4$, 1 of $CaSO_4$, the remainder consisting mainly of SiO_2, Al_2O_3, and Fe_2O_3. The water passing through the box at the time of the examination contained no trace of barium or of strontium.

Another similar deposit was found by Dr. Richardson ('Brit. Assoc. Report,' 1863) to contain about 90 per cent. of barium sulphate and 3 of calcium sulphate, the remainder consisting of silica, alumina, ferric oxide, and moisture.

Professor Lebour states the pipes which convey water from the colliery workings of the Newcastle district are frequently entirely blocked in a short space of time with deposits, amongst which barium sulphate is seldom absent, and is often the chief constituent; and he draws attention to the fact that veins of barium sulphate are by no means uncommon in the coal measures of that district.

This deposition of barium sulphate is of interest in connexion with the discovery of the sulphate as a cementing material in sandstone near Nottingham, and as no complete analysis existed of the water-box deposits in the Durham College Museum, Professors

Bedson and Lebour have kindly furnished me with sections for chemical examination. The following results were yielded by the analyses:—

	Harton, box-deposit.	Jane Pit, Walker-deposit.	Newsham, box-deposit.
Loss at 100°	0·83	0·26	0·39
Loss by ignition	2·15	1·51	1·95
$Fe_2O_3 + Al_2O_3$	5·44	0·12	0·37
BaO	56·72	61·30	61·09
SrO	trace	0·35	0 09
CaO	1·09	0·70	0·82
MgO	0·12	0·14	trace
SO_3	31·10	33·80	32·82
SiO_2	1·19	0·53	0·22
Alkalis, &c. (by difference)	1·86	1·29	2·25
$BaSO_4$	86·37	93·35	93·03

The above deposits were mainly buff-coloured with thin layers of brown interposed; they were soft and loose, powder being easily detached by rubbing the surface with the finger. There was no appearance of crystallisation, but every indication of the deposit having been formed by rapid precipitation.

Professor Bedson ('Journ. Soc. Chem. Industry,' vol. 6, p. 712) and others have found barium chloride to be a common constituent of colliery waters of the district in which the above deposits have been formed. Relatively large quantities of the chloride have been found in some of these samples. The deposition may possibly arise from the admixture with such water of water containing sulphuric acid or ferrous sulphate, or both; since these substances are constantly formed by the oxidation of pyrites in the coal or in the associated shale beds. Or the barium sulphate occurring in veins in the coal measures may pass into solution under conditions yet unknown, and be deposited again as such.

Bischof mentions that hot springs may contain $BaCO_3$ and Na_2SO_4 together in solution; since at high temperatures these substances do not undergo double decomposition. Such a spring water will, however, deposit $BaSO_4$ as it cools; since at ordinary atmospheric temperatures $BaSO_4$ and Na_2CO_3 are produced by the interchange of constituents. Possibly alteration of temperature may in a similar manner give rise to the above remarkable water-box and pipe deposits.

Possibly the deposition of barium sulphate in the form of stalactite, which has occurred in some parts of Derbyshire, may be due to the same causes as these mine-water incrustations.

XVI. "Protoplasmic Movements and their Relation to Oxygen Pressure." By James Clark. Communicated by Professor Vines, F.R.S. Received June 19, 1889.

(Abstract.)

That the presence of free oxygen is one of the essential conditions of protoplasmic movements has long been recognised. Further than this however the subject has not hitherto been investigated.

The following are the results of a long series of experiments made to ascertain the minimum pressure of oxygen necessary to restore the streaming, amœboid and ciliary movements of protoplasm after they have come to rest in the absence of that gas. The object experimented upon was in each case placed in a hanging drop of water and exposed to an indifferent gas such as hydrogen or nitrogen, or else put into connexion with the exhausted receiver of an air-pump. In the former case when the observed movement had ceased a current of indifferent gas containing a definite percentage of oxygen was passed over the object; in the latter a small quantity of air was admitted and the pressure registered. By varying in successive experiments the percentage of oxygen mixed with the diluent gas, and the quantity of air admitted into the air-pump, the minimum pressure of oxygen necessary to restore movement could be ascertained by both methods, so that one could act as a check upon the other.

In this way the minimum for the streaming movement in the plasmodia of Myxomycetes, and in the cells of hairs, of parenchyma, of xylem, phloëm and cambium was found to vary from 1 mm. to over 3 mm. It was lowest for the plasmodia of Myxomycetes, the minimum for *Chondrioderma difforme* being 1 mm., and for *Didymium farinaceum* 1·2 mm. Except with very old plasmodia the results obtained even with unfavourable specimens rarely exceeded 2 mm. With the vegetable cell the variation was much more extensive. In such a favourable object as the root hairs of *Trianea bogotensis* a minimum of 1·2 mm. was occasionally obtained, whereas for the partly cuticularised leaf hairs of *Urtica americana* it sometimes exceeded 3 mm. With the cells of the parenchyma the experimental difficulties were usually very great, and for those of the xylem, phloëm, and cambium still more so. With the former the minimum found for each plant usually lay between 2 mm. and 3 mm., and was sometimes even less. With the latter, cells of all three were found which gave similar results but these were rare, as death of the cell contents usually arrested the experiment. It seems probable however that if variation due to experimental difficulties and the resistance of the cell wall to the passage of oxygen could be eliminated, the minima

for the various tissues of different plants would not vary any more among themselves than do the actual results obtained for naked plasmodia.

The age of the cell or plasmodium and the conditions under which it has been developed to some extent influence the minimum oxygen pressure necessary to restore movement.

The time taken by the protoplasm to recover its streaming movement is too short to be measured in cases where the conditions are favourable, as in young hairs and in slender threads of plasmodia, but increases with cuticularisation of the cell wall, the age of the cell, and the length of time between the cessation of movement and the introduction of the necessary oxygen supply.

Very slight irritation of the plasmodia during the experiment causes them to contract towards definite centres where the protoplasm assumes a more or less spherical condition.

Temporary deprivation of oxygen in a cell showing circulation induces a simplification in the arrangement of the protoplasmic strands. In the leaf cells of Elodea for example the circulation may occasionally pass over into rotation.

After the streaming in plasmodia has been restored by the introduction of the necessary oxygen pressure it ceases again in a very short time. The movement in fact can be maintained only by constant small additions to the oxygen pressure. This is not caused by the consumption of the oxygen in the immediate neighbourhood.

Amœboid movements continue in an atmosphere of hydrogen for some time after the streaming has ceased.

After ciliary movement is arrested in any healthy infusorian by the absence of oxygen the organism soon begins to disintegrate. The introduction of an oxygen pressure of about 1 mm. is sufficient to arrest disintegration and restore ciliary movement, provided the breaking up has not proceeded too far.

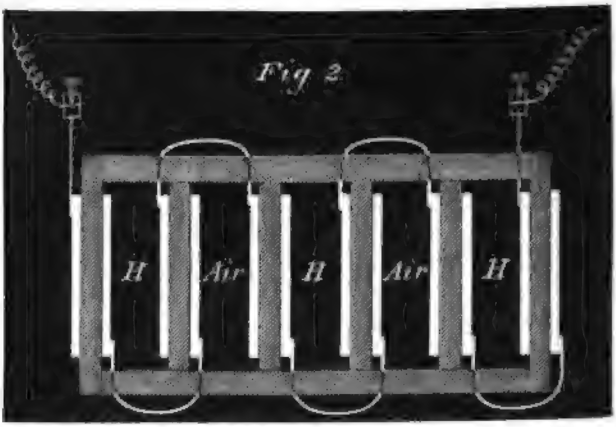
The growth of the plant and the streaming of protoplasm in the active cells thereof appear to be parallel phenomena, streaming, or at least the power of very rapidly assuming the streaming movements, being possessed by the parenchyma and probably the phloëm of plants so long as they continue to grow in an atmosphere of hydrogen. Inability on the part of the protoplasm to continue its movements seems to be always associated with total cessation of growth.

XVII. "Note on the Development of Voltaic Electricity by Atmospheric Oxidation of combustible Gases and other Substances." By C. R. ALDER WRIGHT, D.Sc., F.R.S., Lecturer on Chemistry and Physics, and C. THOMPSON, F.I.C., F.C.S., Demonstrator of Chemistry, in St. Mary's Hospital Medical School. Received June 20, 1889.

About fourteen months ago we had the honour of communicating to the Society ('Roy. Soc. Proc.,' vol. 44, p. 182) the results of a large number of experiments made with electromotor cells, of which a special feature was that one of the plates of the combination consisted of an "aeration plate," or layer of conducting material exposed to the atmosphere, and consequently superficially charged with a film of condensed air, which served as a means of indirectly effecting the oxidation of the other plate (when made of oxidisable metal), or of the fluid surrounding it (when the plate is of non-oxidisable material immersed in an oxidisable fluid). We showed that the E.M.F. of a given combination varies very considerably with the nature of the material of which the aeration plate is made, surfaces of platinum sponge, and especially platinum black, yielding the highest results when the electrolyte is dilute sulphuric acid; a convenient way of constructing the plates being to apply the spongy metal to the surface of unglazed earthenware, or other similar porous non-conducting material, so as to form a conducting film, the electrolytic fluid being absorbed in the porous material and so making contact. We also found that the substitution of pure oxygen for air only increased the E.M.F. by inconsiderable amounts, and that somewhat analogous cells are obtainable by employing combustible gases, *e.g.*, hydrogen, to surround the aeration plate, the opposed plate being either an incorrodible one immersed in a fluid capable of supplying oxygen (permanganate or chromic acid solution, nitric acid, &c.), or one itself capable of parting with oxygen (compressed lead peroxide, &c.). By employing two aeration plates, one in contact with the air and one with the oxidisable gas, a form of gas battery was obtained which, like all other gas batteries, furnished a current considerably less powerful than that due to the chemical action taking place, even under the most favourable conditions.

The most convenient form of simple cell of this class examined by us consisted of a thin plate of unglazed earthenware, such as that used for the porous pots of a Grove's battery, both sides of which were coated over with spongy platinum to within a short distance of he edges, contact being made by means of thin strips of platinum foil pressed against the coated earthenware by means of the clamping

portion of an ordinary binding screw (fig. 1, *aa*). One of the faces of the coated plate was boxed in by means of thin sheet gutta-percha, so as to form a shallow chamber, *b*, into which hydrogen or other gas could be led as required, by means of the inlet and outlet tubes, *c*, *c*; the other face was freely exposed to the air. By impregnating the porous earthenware with dilute sulphuric acid, caustic soda solution, or other electrolyte, and filling the chamber *b* with oxidisable gas (hydrogen, coal-gas, carbon oxide, &c.), maintaining a slow current through it to prevent material alteration of the internal atmosphere by diffusion and osmosis, a form of gas battery was obtained, capable of furnishing continuous currents of sufficient magnitude to effect very appreciable amounts of silver deposition in a silver voltameter. Obviously, by increasing the size of the plates and arranging a number of cells in series, the power might be greatly augmented; to effect this we arranged a series of coated plates in a covered trough or box of insulating material, so as to form partitions, and thus divide the trough into chambers alternately closed in and filled with hydrogen. &c., and open to the air (fig. 2). the connexions being made as indicated, the outer faces of each pair of plates thus being freely exposed to the air, and the inner faces in contact with a hydrogen atmosphere; obviously, to effect this disposition an odd number of chambers is requisite, including those open to the air as well as those



filled with hydrogen, and an even number of doubly-coated porous partitions. We found that the difficulty in avoiding leakage of gases from one chamber to another and various other causes usually prevented the E.M.F. of a battery of n doubly-coated plates from reaching quite as high as n times the E.M.F. obtainable from a single cell; in no case did we obtain as high an E.M.F. as 1 volt per cell, even with only infinitesimal currents, whilst 0·6 to 0·7 volt per cell was about the highest value obtained with currents of magnitude sufficient to measure readily with the silver voltameter. Still our results were sufficiently good to convince us that if the expense of construction were no object, so that large coated plates could be employed, enabling currents of moderate magnitude to be obtained with but small current density, there would be no particular difficulty in constructing "double aeration plate cells" of this kind, competent to yield currents comparable with those derived from ordinary small laboratory batteries; although we concluded that the economical production of powerful currents for commercial purposes by the direct oxidation of combustible gases did not seem to be a problem likely to be readily solved, chiefly on account of the cost of the large appliances that would be requisite.

Precisely the same remark applies to all other forms of cell in which oxidation is effected by means of atmospheric air applied as an "aeration plate," at least, so far as our observations have extended. Given sufficiently large aeration plates, there is no difficulty in setting up aeration cells capable of producing moderately powerful currents, the energy being due either to the oxidation of a metal (*e.g.*, zinc or aluminium, &c.), or to the oxidation of an oxidisable fluid, such as solution of sodium hydrosulphite (Schützenberger's), ammoniacal cuprous oxide, and such like liquids. As yet we have not succeeded in effecting the direct oxidation in this way of alcohol, petroleum,

coal, and such like forms of comparatively cheap sources of energy, but we are far from being convinced that such actions are impracticable.

Our reason for bringing this note before the Society is that at the Conversazione of the Society last night (June 19th) there was exhibited by Mr. Ludwig Mond and Dr. Carl Langer an elegant and compact "dry gas battery," said to have been invented by them, but substantially identical in principle with one of those experimented with by ourselves some two years ago, chiefly differing in being far larger and more neatly finished, and in consequence capable of producing much more current than any arrangement constructed by us. It consisted of a battery of fourteen double aeration plates of films of platinum leaf and platinum black, supported by porous material impregnated with dilute sulphuric acid; when fed with hydrogen and air, as the gases introduced into the compartments formed by the parallel plates arranged in a trough or box, it furnished a current powerful enough to keep alight for a long time a small incandescent lamp, and was stated to be capable of giving a current of 2 ampères per element, with an E.M.F. of about 0·7 volt, the total effective surface of each element being 774 square centimetres.

[*Note.*—Since the above was written, we have had the opportunity of seeing an uncorrected proof of a paper by Mr. Mond and Dr. Langer entitled "On a New Form of Gas Battery" (read before the Society on June 20th, 1889) in which the dry gas battery above referred to is described, as well as various experiments on aeration cells; and the causes discussed which prevent the E.M.F. of such combinations from being as large in practice as it theoretically ought to be, calculating from the heat developed during the chemical actions taking place. From the internal evidence of this paper, as well as from Mr. Mond's assurances to us, we are convinced that the form of gas battery described by Mr. Mond and Dr. Langer was not, as might perhaps be supposed, in any way suggested to them by our previous work (with which indeed they appear to have been entirely unacquainted), but was arrived at by them quite independently. In this paper the authors have repeated unknowingly various of our former experiments on aeration cells, with substantially the same results, as the following figures indicate, obtained with cells where the aeration plates were layers of platinum sponge and black resting on porous plates moistened with dilute sulphuric acid, and opposed to various metals immersed in the acid.

The theoretical values being Zinc = 2·281: Cadmium = 1·924: Copper = 1·203.

Mond and Langer. Nature of aeration plate surface, platinum black. Strength of acid, not stated.		Alder Wright and Thompson.		
		Platinum black.	Platinum sponge.	
		$10H_2SO_4,100H_2O$.	$10H_2SO_4,100H_2O$.	$2\cdot5H_2SO_4,100H_2O$.
Zinc	1·77 volt.	1·750 volt.	1·628 volt.	1·681 volt.
Cadmium	1·425 „	1·505 „	1·383 „	1·356 „
Copper ..	0·70 „	0·780 „	0·658 „	0·636 „

Similarly, in attributing the diminution of the E.M.F. of gas batteries below the calculated amount to the circumstance that heat is evolved during the condensation of gases on surfaces like spongy metals, the authors appear to have been unacquainted with numerous previous papers by one of us, and more especially with one published in 1881, in which this and various allied matters were pretty fully discussed,* and the conclusion arrived at (*inter alia*), that "the heats of condensation of oxygen and hydrogen by platinum, &c., may jointly amount to almost as large a quantity as that developed by their union to form liquid water."—July 30th, 1889.]

XVIII. "On certain Geometrical Theorems. No. 4." By W. H. L. RUSSELL, A.B., F.R.S. Received May 25, 1889.

[Publication deferred.]

XIX. "An Experimental Verification of the Sine Law of Malus." By E. J. SPITTA. Communicated by W. DE W. ABNEY, Capt. R.E., F.R.S. Received May 29, 1889.

XX. "Observations on the Spark Discharge." By J. JOLY. Communicated by Professor G. F. FITZGERALD, F.R.S. Received June 15, 1889.

[Publication deferred.]

Presents, June 20, 1889.

Transactions.

Baltimore:—Johns Hopkins University. Circular. Vol. VIII. No. 73. 4to. *Baltimore* 1889. The University.

* No. III of a series of nine Memoirs on the "Determination of Chemical Affinity in terms of Electromotive Force." 'Proceedings of the Physical Society,' vol. 4, p. 101; also 'Phil. Mag.,' March, 1881 (Series 5, vol. 77, p. 169).

Transactions (*continued*).

Berlin:—Deutsche Chemische Gesellschaft. Berichte. 1889. Nos. 1–6. 8vo. *Berlin.* The Society.

Königliche Technische Hochschule. Die Aufgaben der Hydrotechnik: Rede zum Geburtsfeste Seiner Majestät des Kaisers und Königs Wilhelm II. 8vo. *Berlin* 1889. The School.

Bologna:—Università. Il Gonfalone della Regia Università di Bologna. Folio. *Bologna* 1889. The Contessa Tattini.

Bremen:—Naturwissenschaftlicher Verein. Abhandlungen. Bd. X. Heft 3. 8vo. *Bremen* 1889. The Verein.

Brussels:—Académie Royale de Médecine. Bulletin. Sér. 4. Tome II. No. 11. Tome III. Nos. 1–4. 8vo. *Bruxelles* 1888–89. The Academy.

Académie Royale des Sciences. Bulletin. Sér. 3. Tome XVI. Nos. 11–12. Tome XVII. Nos. 1–4. 8vo. *Bruxelles* 1888–89. The Academy.

Calcutta:—Asiatic Society of Bengal. Journal. Vol. LVI. Part 2. No. 5. Vol. LVII. Part 2. No. 4. 8vo. *Calcutta* 1888–89; Proceedings. 1888. Nos. 9–10. 8vo. *Calcutta* 1889. The Society.

Cambridge, Mass.:—Harvard College. Museum of Comparative Zoology. Bulletin. Vol. XVI. No. 4. Vol. XVII. No. 3. 8vo. *Cambridge* 1889. The Museum.

Harvard University. Bulletin. May, 1889. 8vo. *Cambridge* The University.

Catania:—Accademia Gioenia di Scienze Naturali. Bullettino Mensile. 1889. Fasc. 6. 8vo. *Catania.* The Academy.

Copenhagen:—K. Danske Videnskabernes Selskab. Oversigt. 1888. No. 3. 1889. No. 1. 8vo. *Kjöbenhavn*; Skrifter (Natur. og Math.). Raekke 6. Bd. IV. Afd. 8. 4to. *Kjöbenhavn* 1888; E Museo Lundii. Bd. I. 4to. *Kjöbenhavn* 1888. The Academy.

Cracow:—Académie des Sciences. Bulletin International. Comptes Rendus des Séances de l'Année 1889. Nos. 1–4. 8vo. *Cracovie* 1889. The Academy.

Florence:—R. Biblioteca Nazionale Centrale. Bollettino delle Pubblicazioni Italiane. 1889. Gennaio—Maggio. 8vo. *Firenze*; Codici Palatini. Vol. I. Fasc. 8. 8vo. *Roma* 1888. The Library.

Hackney:—Catalogue of the Tyssen Library in the Town Hall, Hackney. 1888. 8vo. *London.* The Library Committee.

Halle:—Kais. Leopold.-Carol. Deutsche Akademie der Naturforscher. Verhandlungen. Bd. LII. 4to. *Halle* 1888; Leopoldina. Heft 24. 4to. *Halle* 1888. The Academy.

Transactions (*continued*).

Hertfordshire Natural History Society and Field Club. Transactions. Vol. V. Part 4. 8vo. *London* 1889. The Society.

Kew:—Royal Gardens. Bulletin of Miscellaneous Information. No. 30. 8vo. *London* 1889. The Director.

Leipsic:—Königl. Säch. Gesellschaft der Wissenschaften. Abhandlungen (Math.-Phys. Classe) Bd. XV. No. 5. 8vo. *Leipzig* 1889; Berichte (Math.-Phys. Classe). 1889. Heft 1. 8vo. *Leipzig.* The Society.

London:—Chemical Society. Journal. 1889. January to June. 8vo. *London*; Abstracts of Proceedings. Nos. 61–69. 8vo. [*London*] 1889. The Society.

Geological Society. Abstracts of the Proceedings. Nos. 531–542. 8vo. [*London*] 1889. The Society.

Institution of Civil Engineers. Abstracts of the Proceedings. Session 1888–89. Nos. 5–14. 8vo. [*London*]. The Institution.

Institution of Mechanical Engineers. Proceedings. 1889. No. 1. 8vo. *London.* The Institution.

Linnean Society. Journal (Botany). Nos. 156–157, 165–170, 173. 8vo. *London* 1881–89; Journal. (Zoology) Nos. 121, 132. 8vo. *London* 1889. The Society.

Marine Biological Association. Journal. New Series. No. 1. 8vo. *London* 1889. The Association.

Odontological Society of Great Britain. Transactions. Vol. XXI. No. 7. 8vo. *London* 1889. The Society.

Pharmaceutical Society of Great Britain. Journal and Transactions. 1889. January to June. 8vo. *London.* The Society.

Photographic Society of Great Britain. Journal and Transactions. Vol. XIII. No. 8. 8vo. *London* 1889. The Society.

Royal Agricultural Society of England. Journal. Ser. 2. Vol. XXV. No. 49. 8vo. *London* 1880. The Society.

Royal Astronomical Society. Monthly Notices. Vol. XLIX. Nos. 2–5. 8vo [*London*] 1888–89. The Society.

Royal Geographical Society. Proceedings. 1889. January to June. 8vo. *London.* The Society.

Royal Institute of British Architects. Journal of Proceedings. 1889. January to June. 4to. *London.* The Institute.

Royal Institution. Reports of the Weekly Meetings. 1889. January to June. 8vo. [*London*]. The Institution.

Royal Society. Account of the Proceedings in the Case of Warburton *v.* Childs and Others, in the High Court of Justice. Folio. [*London* 1885.] Mr. J. Evans, Treas. R.S.

Society of Arts. Journal. 1889. January to June. 8vo. *London.* The Society.

Transactions (*continued*).

Society of Biblical Archæology. Proceedings. Vol. XI. Part 7. 8vo. *London* 1889. The Society.

Society of Chemical Industry. Journal. 1889. January to June. 8vo. *London.* The Society.

Zoological Society. Proceedings. 1889. Part I. 8vo. *London.* The Society.

Manchester:—Geological Society. Transactions. Vol. XX. Part 8. 8vo. *Manchester* 1889. The Society.

Mexico:—Sociedad Científica "Antonio Alzate." Memorias. Tomo II. Núm. 7. 8vo. *México* 1889. The Society.

Paris:—Académie des Sciences. Comptes Rendus. 1889. Janvier—Juin. 4to. *Paris.* The Academy.

Société de Biologie. Comptes Rendus. 1889. Janvier—Juin. 8vo. *Paris.* The Society.

Société de Géographie. Comptes Rendus. 1888. Nos. 16–17. 1889. Nos. 1–10. 8vo. *Paris.* The Society.

Société d'Encouragement pour l'Industrie Nationale. Bulletin. 1889. Janvier—Juin. 4to. *Paris*; Compte Rendu des Séances. 1889. Janvier—Juin. 8vo. *Paris.* The Society.

Société Française de Physique. Résumé des Communications. 1889. Janvier—Juin. 8vo. *Paris.* The Society.

Société Philomathique. Compte-rendu Sommaire des Séances. 1889. Janvier—Juin. 8vo. *Paris.* The Society.

Philadelphia:—Academy of Natural Sciences. Proceedings. 1888 (pp. 385–464). 1889 (pp. 1–88). 8vo. [*Philadelphia*]. The Academy.

Franklin Institute. Journal. 1889. January to June. 8vo. *Philadelphia.* The Institute.

Pisa:—Società Toscana di Scienze Naturali. Processi-verbali. 1889. Gennaio. 8vo [*Pisa*]; Commemorazione di Giuseppe Meneghini. 8vo. *Pisa* 1889. The Society.

Rome:—Accademia Pontificia de' Nuovi Lincei. Atti. Anno XXXIX. Sessione 5-7. 4to. *Roma* 1886; Processi-verbali. Anno XLII. Sessione 1-4. 12mo. *Roma* 1888-89. The Academy.

Reale Accademia dei Lincei. Rendiconti. Ser. 4. Vol. IV. Fasc. 6–12. Vol. V. Fasc. 1–4. 8vo. *Roma* 1888–89. The Academy.

St. Louis:—Academy of Science. Transactions. Vol. V. Nos. 1–2. 8vo. *St. Louis* 1888. The Academy.

Siena:—R. Accademia dei Fisiocritici. Atti. Ser. 4. Vol. I. Fasc. 3. 8vo. *Siena* 1889. The Academy.

Stockholm:—Kongl. Vetenskaps-Akademie. Öfversigt. Årg. 46. No. 4. 8vo. *Stockholm* 1889. The Academy.

Transactions (*continued*).

Sydney:—Linnean Society of N.S.W. Proceedings. Ser. 2. Vol. III. Part 4. 8vo. *Sydney* 1889; Abstract of Proceedings. 1888. November to December. 1889. January to April. 8vo. *Sydney.* The Society.

Trondhjem:—Kongel. Norske Videnskabers Selskab. Skrifter. 1874-75, 1884. 8vo. *Throndhjem* 1875-6, 1885. The Society.

Venice:—Reale Istituto Veneto di Scienze, Lettere ed Arti. Atti. Ser. 6. Tomo VI. Disp. 10. Tomo VII. Disp. 1-2. 8vo. *Venezia* 1887-89. The Institute.

Vienna:—Anthropologische Gesellschaft. Mittheilungen. Bd. XIX. Hefte 1-2. 4to. *Wien* 1889. The Society.

Kais. Akademie der Wissenschaften. Denkschriften (Math.-Naturw. Classe). Bd. LIV. 4to. *Wien* 1888; Sitzungsberichte (Math.-Naturw. Classe). Abth. 1. Bd. XCVII. Hefte 1-5; Ditto. Abth. 2.a. Bd. XCVII. Hefte 1-7; Ditto. Abth. 2.b. Bd. XCVII. Hefte 1-7; Ditto. Abth. 3. Bd. XCVII. Hefte 1-7. 8vo. *Wien* 1888; Sitzungsberichte (Phil.-Hist. Classe). Bd. CXVI. 8vo. *Wien* 1888; Almanach. 1888. 8vo. *Wien.* The Academy.

K. K. Geographische Gesellschaft. Mittheilungen. 1888. 8vo. *Wien.* The Society.

K. K. Geologische Reichsanstalt. Jahrbuch. Bd. XXXVIII. Heft 4. 8vo. *Wien* 1889. The Institute.

K. K. Naturhistorisches Hofmuseum. Annalen. Bd. IV. No. 2. 8vo. *Wien* 1889. The Museum.

Observations and Reports.

Berlin:—K. Preussiches Geodätisches Institut. Lotabweichungen in der Umgebung von Berlin. 4to. *Berlin* 1889. The Institute.

Brooklyn:—Brooklyn Library. Thirty-first Annual Report. 8vo. *Brooklyn* 1889. The Library.

Calcutta:—Meteorological Office. Report on the Meteorology of India in 1887. 4to. *Calcutta* 1889. The Office.

Cordova:—Observatorio Nacional Argentino. Resultados. Vol. X. 4to. *Buenos Aires* 1888. The Observatory.

Dublin:—General Register Office. Weekly and Quarterly Return of Births and Deaths. January to June, 1889. 8vo. *Dublin.* The Registrar-General.

Hongkong:—Observatory. Observations made in 1888. Folio. *Hongkong* 1889. The Observatory.

London:—Meteorological Office. Daily Weather Report. January to June, 1889. 4to. *London*; Weekly Weather Report. 1889.

Observations, &c. (*continued*).

Nos. 19–22. 4to. *London*; Summary of the Observations made at the Stations included in the Daily and Weekly Weather Reports for the Calendar Month. January to February, 1888. February to March, 1889. 4to. *London*. The Office.

Science and Art Department. Report to the Department on the Action of Light on Water Colours. 8vo. *London* 1888. The Department.

Melbourne:—Department of Mines and Water Supply. Reports of the Mining Registrars, quarter ended 31st December, 1888. Folio. *Melbourne* [1889]. The Department.

Mersey:—Report on the Present State of the Navigation of the River Mersey. 1888. By Admiral Sir G. H. Richards, F.R.S. 8vo. *London* 1889. The Author.

Stonyhurst:—College Observatory. Results of Meteorological, Magnetical, and Solar Observations. 1888. 8vo. *Market Weighton* 1889. The Rev. S. J. Perry, S.J., F.R.S.

Zürich:—Schweizerische Geodätische Commission. Das Schweizerische Dreiecknetz. Bd. IV. 4to. *Zürich* 1889. The Commission.

Washington:—U.S. Patent Office. Official Gazette. April 23rd, 1889, to May 28th. 8vo. *Washington*. The Office.

Journals.

American Chemical Journal. Vol. XI. Nos. 1–4. 8vo. *Baltimore* 1889. The Editor.

American Journal of Mathematics. Vol. XI. Nos. 2–3. 4to. *Baltimore* 1888. The Editors.

American Journal of Philology. Vol. X. No. 1. 8vo. *Baltimore* 1889. The Editor.

American Journal of Science. January to June, 1889. 8vo. *New Haven*. The Editors.

Analyst (The) January to June, 1889. 8vo. *London*. The Editor.

Annalen der Physik und Chemie. 1889. Nos. 1–6. 8vo. *Leipzig* 1889; Namenregister. Bd. I–XXXV. 8vo. *Leipzig* 1889; Beiblätter. 1889. Nos. 1–5. 8vo. *Leipzig*. The Editor.

Archives Néerlandaises des Sciences Exactes et Naturelles. Tome XXIII. Livr. 2. 8vo. *Harlem* 1889. Société Hollandaise des Sciences.

Asclepiad (The) Vol. VI. No. 22. 8vo. *London* 1889. Dr. Richardson, F.R.S.

Astronomie (L') Janvier—Juin, 1889. 8vo. *Paris*. The Editor.

Journals (*continued*).

Ateneo Veneto. Revista Mensile di Scienze, Lettere ed Arti. Ser. 9. Vol. II. Num. 5–6; Ser. 10. Vol. II. Num. 6; Ser. 11. Vol. I. Num. 1–6. Vol. II. Num. 1–5; Ser. 12. Vol. I. Num. 1–6. Vol. II. Num. 1–4. 8vo. *Venezia* 1886–88. Realo Istituto Veneto.

Athenæum (The) January to June, 1889. 4to. *London.* The Editor.

Builder (The) January to June, 1889. Folio. *London.* The Editor.

Canadian Record of Science. Vol. III. No. 6. 8vo. *Montreal* 1889. Natural History Society, Montreal.

Chamber of Commerce Journal. January to June, 1889. 4to. *London.* The London Chamber of Commerce.

Chemical News (The) January to June, 1889. 8vo. *London.* Mr. W. Crookes, F.R.S.

Cosmos. Janvier—Juin, 1889. 8vo. *Paris.* M. l'Abbé Valette.

Educational Times (The) January to June, 1889. 4to. *London*; Mathematical Questions and Solutions from the "Educational Times." Edited by W. J. C. Miller, B.A. 8vo. *London* 1889. The College of Preceptors and The Mathematical Editor.

Electrical Engineer (The) January to June, 1889. Folio. *London.* The Editor.

Electrical Review (The) January to June, 1889. Folio. *London.* The Editor.

Electrician (The) January to June, 1889. Folio. *London.* The Editor.

Epigraphia Indica, and Record of the Archæological Survey of India. Parts 1–2. 4to. *Calcutta* 1888–89. The India Office.

Horological Journal (The). Vol. XXXI. No. 370. 8vo. *London* 1889. British Horological Institute.

Industries. January to June, 1889. 4to. *London.* The Editor.

Meteorologische Zeitschrift. Januar—Juni, 1889. Small folio. *Berlin.* Oesterreichische Gesellschaft für Meteorologie.

Mittheilungen aus der Zoologischen Station zu Neapel. Bd. IX. Heft 1. 8vo. *Berlin* 1889. Dr. Dohrn.

Morskoi Sbornik. [*Russian.*] January to June, 1889. 8vo. *St. Petersburg.* Compass Observatory, Cronstadt.

Naturalist (The) No. 167. 8vo. *London* 1889. The Editors.

Nature. January to June, 1889. Roy. 8vo. *London.* The Editor.

New York Medical Journal. January to June, 1889. 4to. *New York.* The Editor.

Journals (*continued*).

Notes and Queries. January to June, 1889. 4to. *London.* The Editor.

Observatory (The) January to June, 1889. 8vo. *London*; Companion to "The Observatory." No. 144. 8vo. *London.* The Editors.

Revista Científica Mensual de la Universidad Central de Venezuela. Tomo I. 4to. *Carácas* 1887–88. The University.

Revista do Observatorio. Anno 4. Num. 4. 8vo. *Rio de Janeiro* 1889. The Imperial Observatory, Rio de Janeiro.

Revue Internationale de l'Électricité. Janvier—Juin, 1889. 8vo. *Paris.* The Editor.

Revue Scientifique. Avril—Juin, 1889. 4to. *Paris.* The Editor.

Scientific Memoirs by Medical Officers of the Army of India. Edited by Sir B. Simpson, M.D. Parts 1–4. 4to. *Calcutta* 1885–89. The Indian Government.

Stazioni Sperimentali Agrarie Italiane (Le) Vol. XVI Fasc. 4. 8vo. *Roma* 1889. Prof. Pasquale Freda.

Symons's Monthly Meteorological Magazine. January to June, 1889. 8vo. *London.* Mr. Symons, F.R.S.

Victorian Year-Book for 1887–88. 8vo. *Melbourne* 1888. The Victorian Government.

Zeitschrift für Biologie. Bd. XXV. Hefte 3–4. Bd. XXVI. Heft 1. 8vo. *München* 1888–89 The Editors.

Dawson (G. M.) Notes on the Indian Tribes of the Yukon District and Adjacent Northern Portion of British Columbia. 8vo. [*Montreal*] 1887. The Author.

Gomez Vidal (Don Manuel) Magnetismo Universal. 8vo. *Madrid* 1889. The Author.

Jeppe (F.) Map of the Transvaal or South African Republic, and surrounding Territories. [Four Sheets.] *Pretoria* 1889. The Compiler.

Kölliker (A.), For. Mem. R.S. Handbuch der Gewebelehre des Menschen. Bd. I. 8vo. *Leipzig* 1889. The Author.

Lorange (A. L.) Den Yngre Jernalders Svaerd: et Bidrag til Vikingetidens Historie og Teknologi. 4to. *Bergen* 1889. The Museum, Bergen.

Monaco (Prince Albert de) Le Dynamomètre à ressorts emboîtés de l'*Hirondelle*. 8vo. *Paris* 1889. With one other Excerpt in 8vo. The Author.

Raum (J.) Der gegenwärtige Stand unserer Kenntnisse über den Einfluss des Lichtes auf Bacterien und auf den thierischen Organismus. 8vo. [*Warschau*] 1889. The Author.

Schorlemmer (C.), F.R.S. Der Ursprung und die Entwickelung der Organischen Chemie. 8vo. *Braunschweig* 1889. The Author.

Vernon-Harcourt (L. F.) Some Canal, River, and other Works in France, Belgium, and Germany. 8vo. *London* 1889. The Author.

Williamson (B.), F.R.S., and F. A. Tarleton. An Elementary Treatise on Dynamics. 2nd Edition. 8vo. *London* 1889. The Authors.

Medallion Portrait of Mr. John Evans, Treas. R.S. Mr. Evans.

Eleven mounted Photographs of Nebulæ, &c. Mr. Isaac Roberts, F.R.A.S.

NOTICES TO FELLOWS OF THE ROYAL SOCIETY.

A Monograph of the Horny Sponges, by Dr. R. von Lendenfeld, is now published (see above). The price to the public is £3; to Fellows of the Royal Society, £2. As the edition is limited to 500 copies, Fellows should order their copies at once of the Assistant Secretary.

The following are now offered *to Fellows only* at reduced prices :—
"Catalogue of the Scientific Books in the Library of the Royal Society"—

Part 1.	Transactions, Journals, &c......	4*s*. net.
Part 2.	General Catalogue............	7*s*. ,,
	The two Parts........................	10*s*. ,,

"International Polar Expedition. Fort Rae Observations." 10*s*. 6*d*. net.
"Catalogue of Scientific Papers," in sets :—

	Net prices. £	*s.*	*d.*	
Vols. I—VI in Half-Morocco at	3	10	0	the set.
*Vol. I, Half-Morocco, and II—VI, Cloth. .	2	14	6	,,
Vols. VII, VIII in Half-Morocco at . .	1	10	0	the two.
,, ,, ,, in Cloth	1	1	0	,,

The Library is now open from 11 A.M. to 6 P.M. (Saturdays 1 P.M.).

Philosophical Transactions.—The Fellows are now permitted to give standing orders for the delivery of the volumes of 'Philosophical Transactions' to their addresses as published. The order must be in writing, and should be sent to the Assistant Secretary.

On application to Messrs. Harrison and Sons, 46, St. Martin's Lane, Fellows and others can have the separate numbers of the 'Philosophical Transactions' bound in cloth case for 2*s*. 6*d*., or can obtain cloth cases for binding, price 1*s*. 6*d*.

List of Fellows.—Fellows who desire to have the Annual List posted to them every year as soon as issued should send their names to the Assistant Secretary.

A printed post-card of the papers to be read at each meeting will be sent weekly to any Fellow upon application to Messrs. Harrison and Sons, 46, St. Martin's Lane, W.C.

* Vol. I in cloth is sold out.

HARRISON AND SONS, 45 & 46, ST. MARTIN'S LANE, W.C.,

AND ALL BOOKSELLERS.

PROCEEDINGS OF

THE ROYAL SOCIETY.

VOL. XLVI. No. 284.

CONTENTS.

November 21, 1889.

For continuation of Contents see 2nd page of Wrapper.

Price Three Shillings and Sixpence.

20/1/90.

	1st Hundred. 12th November, 1879, to 29th September, 1880.	2nd Hundred. 29th September, 1880, to 15th October, 1881.	3rd Hundred. 18th October, 1881, to 27th June, 1882.	4th Hundred. 1st July, 1882, to 28th August, 1883.	5th Hundred. 30th August, 1883, to 23rd June, 1884.	6th Hundred. 24th June, 1884, to 12th February, 1885.	7th Hundred. 18th February, 1885, to 24th August, 1885.	8th Hundred. 25th August, 1885, to 2nd August, 1887.	9th Hundred (First Half). 2nd August, 1887, to 20th February, 1888.
5473·2								—	
5454·7	—								
5445·9	—	—							
5444·2	—							—	
5428·8	—	—							
5404·8	—	—			—			—	
5403·1	—	—						—	
5396·1		—							
5392·2								—	
5390·4								—	
5388·5								—	
5370·5	—	—							
5369·0	—	—						—	
5366·5		—							
5364·0		—							
5361·9		—						—	
5352·4	—								
5348·6	—								
5340·2	—	—							
5339·2	—	—							
5327·3	—	—							
5323·4	—	—							
5306·3								—	
5287·3								—	—
5282·4								—	
5272·5	—							—	
5269·5	—	—	—	—	—				
5268·5	—	—	—	—	—				
5265·8	—	—	—						
5262·4	—	—							
5252·5								—	
5232·1	—	—							
5229·0								—	
5226·2	—	—							
5207·6	—	—	—	—	—				
5208·7	—	—	—	—	—				
5201·5	—	—		—					
5194·1	—								
5191·7	—	—							
5185·0								—	—

TABLE B.—NICKEL.

Region F—*b*.

	1st hundred.	2nd hundred.	3rd hundred.	4th hundred.	5th hundred.	6th hundred.	7th hundred.	8th hundred.	9th hundred (first half).
5155·1			—	—	—		—		
5145·7	—	—	—	—				—	
5141·8	—	—						—	
5136·8	—	—	—					—	
5098·5		—							
5080·6		—	—	—		—			
5079·8		—	—	—					
5034·6	—	—							
5016·8		—						—	
4983·5	—	—							
4979·4								—	
4935·1								—	—

Region *b*—D.

TABLE C.—TITANIUM.

Region F—*b*.

	1st hundred.	2nd hundred.	3rd hundred.	4th hundred.	5th hundred.	6th hundred.	7th hundred.	8th hundred.	9th hundred (first half).
5151·2		—	—						
5147·0			—	—	—				
5144·5				—	—	—			
5119·9			—					—	—
5086·5		—							
5064·4	—		—						
5038·7		—	—						
5038·0				—	—				
5037·8		—	—		—				
5035·8	—	—	—	—					
5035·2	—	—	—						
5019·2								—	—
5013·3		—							
5006·6	—							—	—

	1st hundred.	2nd hundred.	3rd hundred.	4th hundred.	5th hundred.	6th hundred.	7th hundred.	8th hundred.	9th hundred. (first half).
4981·0	—	—							—
4964·5		—						—	
4913·2		—						—	
4884·2		—							
4869·5		—							

Region *b*—D.

	1st hundred.	2nd hundred.	3rd hundred.	4th hundred.	5th hundred.	6th hundred.	7th hundred.	8th hundred.	9th hundred. (first half).
5865·2								—	—
5738·0			—	—				—	
5701·5		—		—			—		
5689·3									—
5688·5		—							
5674·4			—						
5513·2	—	—	—		—			—	
5511·8		—							
5502·8								—	
5489·0								—	—
5473·2								—	
5447·3				—				—	
5425·0							—	—	
5418·0								—	—
5408·6								—	
5403·1	—	—						—	
5396·1					—				
5335·9								—	
5296·7	—	—	—	—					
5295·5			—						
5271·5		—							
5255·0			—			—	—		
5246·3	—	—							
5226·0				—	—				
5224·6	—		—			—		—	
5224·2	—		—			—	—	—	
5209·5	—	—	—		—			—	—
5207·3									—
5205·5					—				
5192·3	—								
5188·3	—	—							

Table D.—Unknown Widened Lines.

Region F—*b*.

	1st hundred.	2nd hundred.	3rd hundred.	4th hundred.	5th hundred.	6th hundred.	7th hundred.	8th hundred.	9th hundred (first half).
5179·4								1	
5175·0							3		
5172·2								1	
5171·1								2	
5170·0								2	
5164·0									6
5162·2	1		23	49	21	30			
5162·0			9	7	61	67	62	47	3
5160·4		1		5			4		
5160·0			1	4		9			
5159·5	1		31	59	80	86	57	56	39
5159·0			1	8	18	11	41		1
5157·2				4					
5156·5					8				
5156·0	1	12	37	4	82	91	95	53	8
5155·4					1				
5154·0							1		
5153·8					1				
5151·8					1				
5150·0				1					
5149·8		8	2	8		8			
5149·5				4			29	12	
5149·2				1					
5149·0	2	32	31	36	4		35		
5148·8			1	2					
5148·0							1		
5146·5				2					
5146·0			36	12					
5145·5						1			
5144·5					1				
5144·2	1	3		2					
5143·2			2						
5143·0							20	3	
5142·8		21	7	19	2				
5142·2	18	4		1					
5141·5								1	1
5141·2								1	1
5140·4		2							
5139·4		1	2	3					
5139·0					1		1	1	
5138·0					1		3		
5137·8		12	35	64	13	10	3	3	
5137·5			4		72	79	22	14	
5137·0			2	2	1				
5136·5			3	1					
5136·0			4		9	22	27	2	
5135·8			37	52	13	2			
5135·5		33	15		58	36	20	21	
5135·0					16	86	11		

	1st hundred.	2nd hundred.	3rd hundred.	4th hundred.	5th hundred.	6th hundred.	7th hundred.	8th hundred.	9th hundred (first half).
5134·4						19			
5134·0					12	41	10		
5133·8		80	47	43	62	3	27	17	
5133·5			1		1	3	17	2	
5132·8				3					
5132·5			1						
5132·0		14	21	6				2	
5130·0			1						
5129·6		17	19	4					
5128·8							1		
5127·5			1						
5127·0				1					
5119·3								1	
5118·0	4		14						
5117·3								2	
5116·2			7						
5116·0	3	6	24	3					
5115·5							9	1	
5112·5									12
5112·1		6	22	4	2	1		62	27
5108·6								1	
5103·5					1				
5101·0							1	1	
5095·0								2	
5094·0								2	
5092·0								2	
5089·0				1					
5088·6		1							
5088·1		1							
5087·7		1							
5086·8		1							
5086·0			17		1				
5084·5							2	2	
5084·0		1					3		
5083·3		1	2	3					
5083·0						2			
5082·0						2			
5081·8								2	2
5081·5							3		
5080·0				1					
5079·5						2			
5077·0				1					
5070·8		1							
5069·5		1							
5067·0		1							
5065·0			8						
5063·0								1	
5062·8				3		2		1	
5062·4						2		3	
5062·0							5	1	
5061·5							2	6	
5061·0			2				3	2	
5060·1								1	

	1st hundred.	2nd hundred.	3rd hundred.	4th hundred.	5th hundred.	6th hundred.	7th hundred.	8th hundred.	9th hundred. (first half).
5044·6		3						2	
5043·0		1							
5042·3			4						
5042·0			8					1	
5038·9			1	1					
5037·0						1			
5034·8	11		3						
5030·0		1						4	
5028·9		1							
5020·0								1	
5017·2		1							
5009·2									1
4995·9								1	
4975·9								10	19
4971·0								2	
4959·0									5
4944·0			1						
4921·2								4	1
4910·5				2				1	
4912·5								1	
4910·5								1	
4910·0				2					
4900·3								1	
4895·9									1
4893·9									4
4891·8					1				
4888·3	1								
4885·0							1		
4865·0						1			

Region *b*—D.

	1st hundred.	2nd hundred.	3rd hundred.	4th hundred.	5th hundred.	6th hundred.	7th hundred.	8th hundred.	9th hundred. (first half).
5893·0							1		
5890·5							10		
5890·0							1		
5887·5				1			3		
5886·5				1	1		3		
5885·2					1				
5884·5							1		
5882·4								1	
5878·3								1	
5876·5						2			
5870·0						5			
5869·5					1				
5867·5		4		14		8	10	2	
5867·0							10		
5866·0				7	10	5	50	2	
5865·5			1	2			1		
5865·0		8	16	12	1	1	23		
5864·3						1	4		
5864·0					2		2		
5863·2		5						2	2
5863·0					1		6		

	1st hundred.	2nd hundred.	3rd hundred.	4th hundred.	5th hundred.	6th hundred.	7th hundred.	8th hundred.	9th hundred (first half).
5862·0							12		
5860·2							3		
5860·0							8		
5858·4		1							
5857·5							3		
5856·6					1				
5855·2				1	1				
5852·0			4						
5851·5		3	1						
5847·5							4		
5846·5							4		
5846·3		3	4						
5846·0			6						
5840·0								1	
5839·0								2	
5833·0									6
5832·5									1
5832·0								1	
5831·5								1	
5830·0								1	
5829·5				2					
5826·5				2					
5824·0				1					
5821·8								2	
5821·5							2		
5821·0				1					
5819·6					2				
5817·0		1		1					
5816·5		1							
5814·0				3			10		
5813·5			2	1					
5794·0					1				
5777·5		3		7			2		
5776·5		1		1			2		
5772·5				1					
5765·0				4					
5763·5				1					
5760·2					1				
5758·5				2					
5756·0					6			1	3
5748·0				1					
5747·5							2		
5747·0				8					
5746·0					3				
5745·0				1	1				
5744·5			1	6		2			
5743·5			1						
5743·0					1	1			
5742·5				17	9	6			
5742·2				1					
5742·0		2	3	11	7	7	5	1	
5741·5							6	4	
5741·C									1

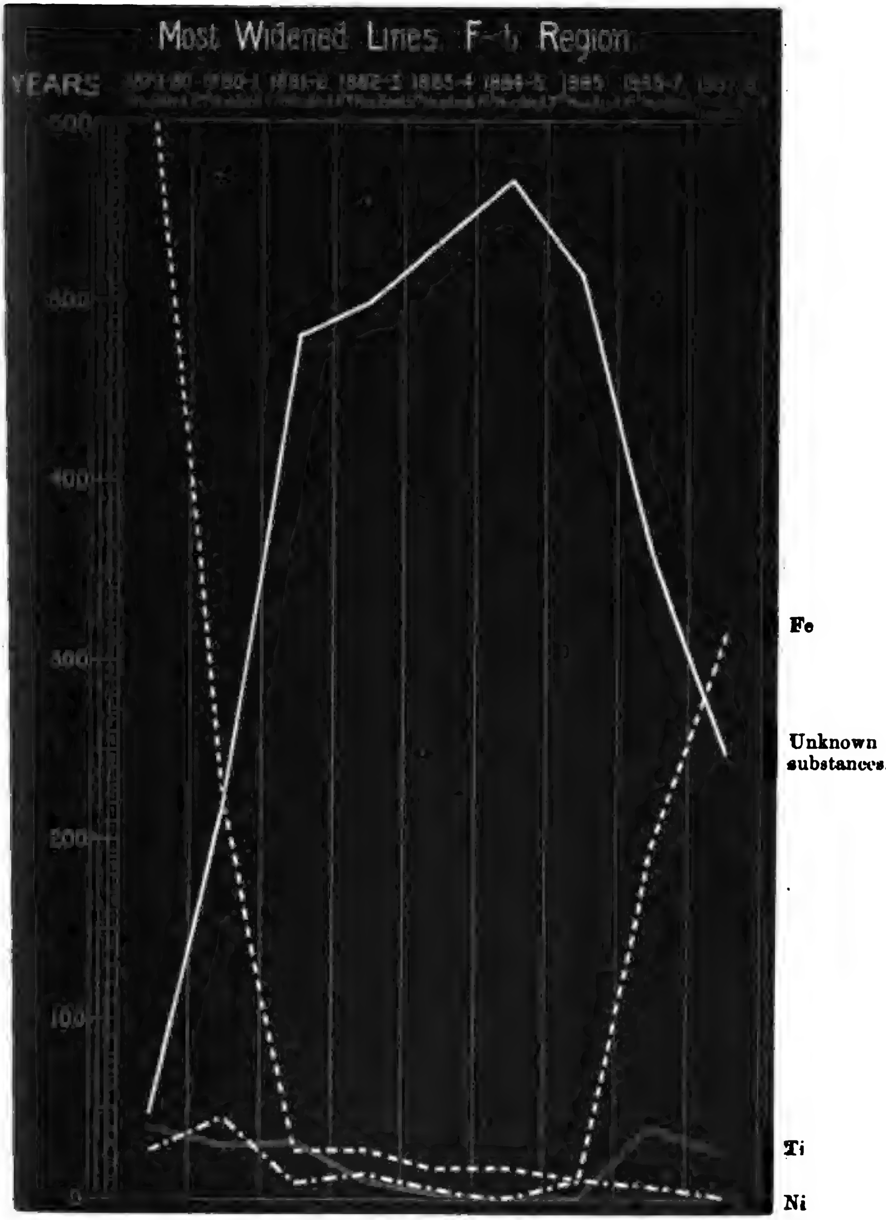
	1st hundred.	2nd hundred.	3rd hundred.	4th hundred.	5th hundred.	6th hundred.	7th hundred.	8th hundred.	9th hundred. (first half).
5738·5				2					
5736·8				1					
5736 5		18	6	49	13				
5756·0				3	9	11	2		
5735·5				3		1		2	
5735·0				1	2		11	6	
5734·5		1	1	6					
5733·0				1					
5732·0					1				
5731·8				1					
5730·0		15	55	38	83	88	73	1	
5729·5				2					
5728·0				5					
5727·5				4				1	
5726·5		13	51	25	26				
5726·0		2	7	8	60	89	72	1	
5724·0							1	1	
5723·0			2	6					
5722·5							4		
5722·0							3		
5721·5				1					
5721·3						8			
5719·0				2				2	
5718·0								1	
5706·0			1						
5703·5								14	1
5703·0				4					4
5702·5								2	
5702·0				2					
5699·0		23	1	10					
5698·5				1	1		4		
5698·0		1							
5697·0				4					
5695·0							4		
5678·0				1					
5677·5				1					
5672·5			1						
5672·0		51	56	64	61	83	32	40	38
5671·5			1	1					
5671·0		51	56	64	62	85	30	40	38
5670·5			1	3					
5670·3							3		
5670·0			1	1		1	1	8	
5669·7							3		
5669·5			2	5					
5660·0				1					
5637·5			1						
5636·2		1							
5629·5			1						
5628·0		17	1				2		
5627·5			19	3					
5627·0			2	2					
5626·7			9						

	1st hundred.	2nd hundred.	3rd hundred.	4th hundred.	5th hundred.	6th hundred.	7th hundred.	8th hundred.	9th hundred. (first half).
5626·5		1		1					
5626·0			9	4					
5622·0						1			
5621·0						1			
5602·0				1					
5595·2								1	
5583·5			2						
5572·4								1	
5567·5									1
5566·3								1	2
5559·0				1					
5558·4		1							
5541·0					1				
5538·0				1					
5537·0		2	5						
5536·8						4			
5536·5						4		1	
5536·2		2	5						
5536·0				1					
5535·5					1				
5535·0					1				
5534·2		1							
5533·8		1						1	
5532·5			1						
5532·0			1						
5529·5		3						1	
5521·0								2	
5520·4								1	
5515·5		1			1				6
5511·4		1							
5505·8					1				
5505·2					1				
5493·6		1							
5492·5		1							
5489·5			1	3					
5486·0		1							
5484·5							3	3	
5484·0								4	
5482·5								1	
5481·0		2						2	
5477·3								3	
5475·0							3		
5466·0			1						
5463·3			6					1	
5462·5				1					
5461·0		9	7	3	1		3		
5460·5								8	
5460·0		21	48	15	21	2	10	1	
5459·5							11	26	2
5459·0		17	26	29	24	65	18		
5457·0								1	
5456·0								1	
5447·0			1						

	1st hundred.	2nd hundred.	3rd hundred.	4th hundred.	5th hundred.	6th hundred.	7th hundred.	8th hundred.	9th hundred (first half).
5444·0		1							
5435·5			6					2	
5434·5			6					2	4
5431·8								3	8
5431·6			6	1					4
5428·9		1							
5428·0								1	
5427·0						1		3	
5426·2				1					
5426·0		21	52	37	51	73	45	17	
5425·3					1		3	11	
5424·5			1						
5424·0							4		
5423·7	1								
5423·5					1				
5419·5						5			
5414·5					2				
5413·2							1	1	
5412·2							1	1	
5410·0		2							
5409·2					2				
5409·0		1							
5406·4								17	6
5406·0								1	
5397·1								1	
5393·5		1					2	23	4
5372·5								5	6
5368·0								2	
5367·5								1	
5365·2								2	
5364·2		1							
5360·5								1	
5357·5					1			1	
5345·0	1							2	
5333·7								2	
5338·0								1	
5331·0				1				3	
5329·2		1							2
5328·6		1							
5328·4		1							
5327·7	14	6			1				
5327·2					1				
5321·3		7	8						1
5320·3		6	8						
5318·5						1			
5307·5		6	8						
5300·0		1						33	48
5299·0								1	
5297·5	2	4	2						
5296·2	1	1	3	1					
5296·0			1						
5281·7								2	
5277·0		1							

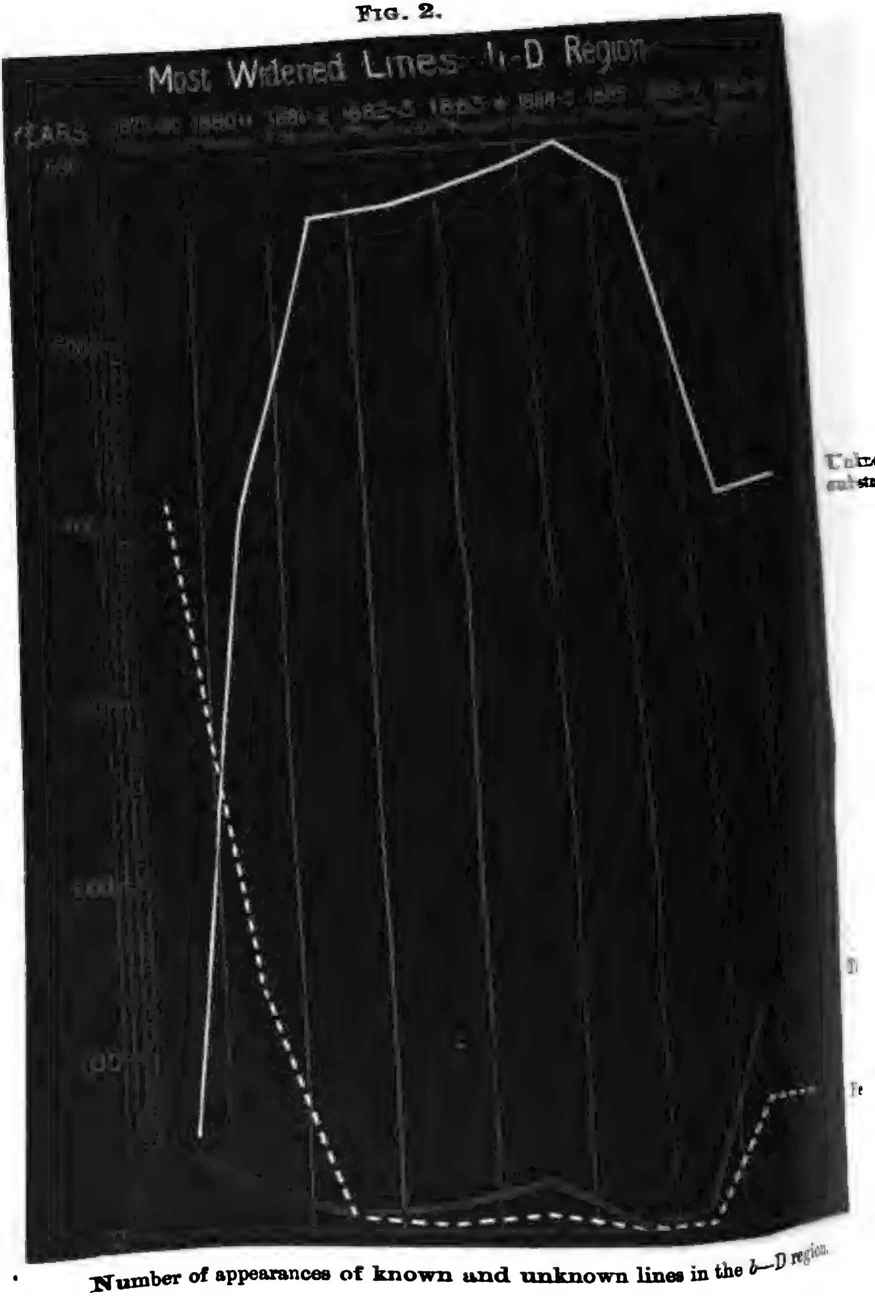
	1st hundred.	2nd hundred.	3rd hundred.	4th hundred.	5th hundred.	6th hundred.	7th hundred.	8th hundred.	9th hundred (first half).
5275·0	1								
5263·4	2	1							
5259·5		2	4	2		8	6	14	
5259·3							6		
5256·0			2						
5255·8							2		
5255·5								1	
5248·0								1	
5246·5			1						
5244·9									1
5240·0			1						
5238·0		1	4	2					
5237·5						4	1	1	
5236·8		1							
5227·5								4	
5227·3	1							1	
5226·5						1			
5224·8						3	4		6
5224·0			2	2	2	3			
5223·8								1	
5223·4		1							
5220·0						1			
5219·5						2			
5219·2	1	5	5						
5219·0			1					2	
5218·7	1	13	2	14					
5218·5					33	6			
5218·0			1			2	1	22	7
5217·1	2	2				1			
5216·5		2				1			
5216·2	2					1			
5215·5	2					1			
5214·5						1			
5214·3	2								
5213·0								2	
5210·0								3	
5209·2							2		
5206·5								4	
5202·5								2	
5200·5								1	
5199·6							1		
5199·4							1	1	
5193·5							1		
5191·8		1							
5190·8	16	5							
5180·9		1							
5179·4								1	
5178·0		2							
5176·0				1					
5172·2								1	
5171·5								2	
5170·5				1					
5170·1		1						2	

Fig. 1.



Number of appearances of known and unknown lines in the F—*b* region.

FIG. 2.



Number of appearances of known and unknown lines in the b—D region.

The relation of the present observations to former ones is shown in the accompanying diagrams (figs. 1 and 2).

[In each observation the six most widened lines in each region are recorded, so that in each 100 observations there are 600 lines in each region. The relative numbers of the lines which are due to iron, nickel, titanium, and unknown substances are graphically represented by the curves. The dotted line refers to the lines of iron, the chain line to those of nickel, the multiple line to those of titanium, and the thick continuous line to those of unknown substances.

The minimum period occurred in 1879, and the maximum at the end of 1883, so that the observations now nearly extend through a Sun-spot cycle.

It will be seen that the conclusion I arrived at in 1886,* namely, that "as we pass from minimum to maximum, the lines of the chemical elements gradually disappear from among those most widened, their places being taken by lines of which we have at present no terrestrial representatives," is supported by the continued observations, especially in the F—*b* region.

The 150 observations now added were made by Messrs. Fowler and Taylor, and reduced and mapped by Messrs. Coppen and Porter.—November 1, 1889.]

II. "On the Cause of Variability in Condensing Swarms of Meteorites." By J. Norman Lockyer, F.R.S. Received June 27th, 1889.

I. The General Theory.

One of the general conclusions I arrived at in my paper on "Researches on the Spectra of Meteorites"† was as follows:—"Most of the variable stars which have been observed belong to those classes of bodies which I now suggest are uncondensed meteor-swarms, or condensed stars in which a central more or less solid condensed mass exists. In some of those having regular periods the variation would seem to be partly due to swarms of meteorites moving round a bright or dark body, the maximum light occurring at periastron."

And again in 1888,‡ referring to the former class, I added, "If the views I have put forward are true, the objects now under consideration are those in the heavens which are least condensed. In this point, then, they differ essentially from all true stars like the Sun. This fundamental difference of structure should be

* 'Roy. Soc. Proc.,' vol. 40, p. 352.

† 'Roy. Soc. Proc.,' vol. 43, p. 154.

‡ 'Roy. Soc. Proc.,' vol. 44, p. 81.

revealed in the phenomena of variability, that is to say, the variability of the bodies we are now considering should be different in kind as well as in degree from that observed in some cases in bodies like the Sun or α Lyræ, taken as representing highly condensed types. There is also little doubt, I think, that future research will show that when we get short period variability in bodies like these, we are here really dealing with the variability of a close companion."

The recent work of Chandler* on the colours of these interesting objects, and the relation of colour to period, furnishes further tests of the theory which I suggested as to their origin.

Variability due to Subsidiary Swarms.

Briefly, this was that in the case of the stars of Group II, which spectroscopic observations show to be composed of uncondensed

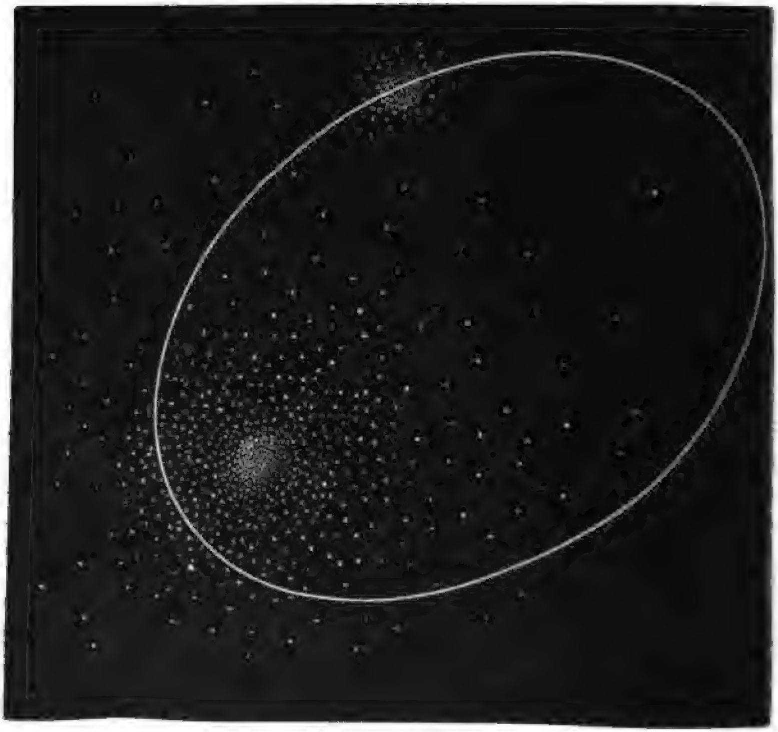


Fig. 1.—Diagram showing the probable origin of variability in condensing swarms.

* 'Astr. Journ.,' No. 179–180.

swarms of meteorites, the variability is produced by the revolution of one or more smaller swarms round the central swarm, the maximum luminosity occurring at periastron passages, when the revolving swarms are most involved in the central one.

Fig. 1 illustrates this suggestion in the simplest case, where there is only one revolving swarm, as in Mira Ceti. The range of variability depends upon the eccentricity of the orbit and the periastron distance of the revolving swarm.

According to this theory, the normal condition is that which exists at minimum, and in this respect it resembles that suggested by Newton, namely, that the increase of luminosity at maximum was caused by the appulse of comets. All other theories take the maximum as the normal condition and the minimum as a reduction of the light by some cause, such as a large proportion of spotted surface or eclipses by dark bodies. In the variables of the Algol type, where the periods are very short, there can be no doubt, after the Henry Draper Memorial photographs, that the eclipse explanation is the true one. But in the variables of Group II, where the period is about a year and the luminosity at maximum in the generality of cases is about 250 times, though in others it runs up to 1600, that at minimum (corresponding to a difference of six magnitudes), it is obvious that the eclipse explanation no longer holds, on account of period, and also that the spotted surface explanation is inadmissable on account of range. If, however, the minimum be taken as the normal condition, and the effects of the revolution of such a swarm as I have assumed be considered, both length of period and range of variability can be explained. In this class of variables the rise to maximum is more rapid than the fall to minimum, and, according to my explanation, the sudden increase is due to the first collision between the two swarms, while the fall to minimum represents the gradual toning down of the disturbance.

Tests of the Theory.

In the Bakerian Lecture (p. 84) I showed how this explanation of variability bore four distinct tests. The first test was that Group II should be more subject to variability than any other group; and I showed that 1 out of every 7 stars of Group II are variable, whilst only 1 in 659 of the stars included in Argelander's catalogue are variable. The other tests were:—(2) when the swarm is least condensed, we shall have the least results from collisions; (3) when it is fairly condensed, the effect at periastron passage (if we take the simplest case, where there is only a single revolving swarm) will be greatest of all; (4) in the most condensed swarms there will be little or no variability, because the outliers of the central swarm may be

drawn entirely within the orbit of the secondary body. I gave tables to show that these tests were satisfied by all the variables included in Dunér's catalogue of red stars.* In the tables which follow, it will be seen that by far the greater number of variables in the group under discussion fall in species 9 and 10, which may fairly be taken to represent the mean condensation, there being in all 15 species. There can, therefore, be no doubt that the three tests just referred to are fully satisfied.

In this paper I propose to further test my theory by the colour observations of Chandler and by the question of irregularity, confining myself to stars known to belong to Group II of which Chandler gives the degree of redness. The stars selected for discussion are the III*a* variables from Gore's revised catalogue.

II. Details of Variables of Group II.

The following tables contain all the particulars of stars with periods varying from 50 to 500 days. Gore's, Chandler's, and Dunér's star numbers are given as well as the star's name. The magnitudes of the variable at maximum and minimum, and also the period, have been taken from Gore.

Colour Notation.

On Chandler's colour scale 0 corresponds to pure white, 1 to white very slightly tinged with yellow, 2 and 3 to deeper yellow tinges, 4 to orange, 5, 6, 7, 8 and 9 to gradually deepening reds, and finally 10 corresponds to the deepest red stars known, such as R Leporis.

The colour notation employed by Dunér is as follows :—

Rrrj	Almost absolute red.
Rrj	Red-yellow foncé.
Rj	Red-yellow.
Jr	Yellow-red.
Jjr	Clear yellow-red.

In the Bakerian Lecture for 1888 I gave a series of tables in which the stars of Group II were classed in different species according to their spectra. I have accordingly given with each variable the number expressing the species to which it belongs. In some cases, the details have not been sufficient to assign the star to a definite species, but have been enough to determine whether it was near the first (Species 1) or the last (Species 15). In such cases, the words "early" or "late" are appended. Where the species of a star is doubtful, the word "indeterminate" expresses that fact.

* 'Les Étoiles à Spectres de la troisième Classe.' (Stockholm, 1884.)

Variables with periods of 50 to 100 days.

Gore.	Chandler.	Dunér.	Name.	Max.	Min.	Period.	Colour.		Species.
							Chandler.	Dunér.	
121	5912	184	*g*(30) Herculis	5	6·2	40—125	3	Jr	8
153	7106	238	S Vulpeculæ	8·85	9·95	67·795	3	Rj	Indeterminate.

Variables with periods of 100 to 200 days.

Gore.	Chandler.	Dunér.	Name.	Max.	Min.	Period.	Colour.		Species.
							Chandler.	Dunér.	
16	845	20	R Ceti	7·9—8·7	< 12·8	167·1	2·4	Jr	9
38	2213	55	η Geminorum	3·2	3·7—4·2	135—151	3	Jr	11
83	4521	127	R Virginis	6·5—7·5	10—10·9	145·7	1·3	Jr	Late.
101	5494	165	S Libræ	8·0	12·5	190	3·0	Rj	Early.
125	5948	—	R Ursæ Min.	8·6	10·5	166	3·2	—	Not in Dunér.
171	7560	261	R Vulpeculæ	7·5—8·5	12·5—13	137·5	2·0	Jr	Indeterminate.
176*a*	7754	266?	W Cygni	5·8—6·2	6·7—7·3	120—138	5	Rj	9

Variables with periods of 200 to 300 days.

Gore.	Chandler.	Dunér.	Name.	Max.	Min.	Period.	Colour.		Species.
							Chandler.	Dunér.	
60	3170	81	S Hydræ	7·5—8·5	< 12·2	256·4	2·1	Jr	Early.
82	4511	125	T Ursæ Maj.	6·4—8·5	13	255·6	2	Jr	Late.
85	4557	128	S Ursæ Maj.	7·2—8·2	10·2—12·8	224·8	3·2	Rj	Indeterminate.
86	4596	—	U Virginis	7·7—8·1	12·2—12·8	207·4	1·1	—	—
94*a*	5194	158	V Bootis	7·0	9·4	266·5	3·6	Rjj	9
95	5190	157	R Camelopardi	7·8—8·6	12?	258·5	2·1	Rrj	Late.
96	5237	159	R Bootis	5·9—7·8	11·3—12·2	223	2·7	Jr	Early.
124	5950	186	W Herculis	8	11·5	281·2	3·2	Rj	9
125*a*	5955	187	R Draconis	7·0—8·7	< 13	244·5	2·0	Jr	14
135	6512	—	T Herculis	6·9—8·3	11·4—12·7	—	1·4	—	—
148	6905	222	R Sagittarii	7·0—7·2	< 12	270	3·6	Rj	Early.

Variables with periods of 300 to 400 days.

Gore.	Chandler.	Dunér.	Name.	Max.	Min.	Period.	Colour. Chandler.	Colour. Dunér.	Species.
11	513	9	R Piscium	7—8·3	< 12·5	345	2	Rj	9
14	806	18	*o* (Mira) Ceti	1·7—5·0	8·7—9·5	331·3	5·9	Rj	10
17	976	23	T Arietis	7·9—8·2	9·4—9·7	324	3·2	Rj	9
23	1577	37	R Tauri	7·4—9·0	< 13	326·6	4·5	Rrj	9
37*a*	2100	—	U (Nova) Orionis	6·1—7·5	< 12	365±	7	—	—
48	2684	68	S Canis Min.	7·2—8·0	< 11	332·2	4·1	Rj	Early.
56	2946	76	R Cancri	6·2—8·3	< 11·7	354·4	5·3	Rj	6
64	3477	91	R Leonis Min.	6·1—7·5	< 11	374·7	6·0	Rrj	10
65	3493	92	R Leonis	5·2—6·7	9·4—10	313	6·9	Brj	10
71	3825	100	R Ursæ Maj.	6·0—8·1	13·2	302·2	1·6	Jr	Early.
74	3984	106	R Crateris	8	9	395	8·1	Brj	Early.
80	4407	118	R Corvi	6·8—7·3	11·5	318·6	3·7	Rj	9
103	5504	166	S Coronæ	6·1—7·8	12·5	360·4	4·9	Rj	8
108	5677	170	R Serpentis	5·6—7·6	< 11	357·6	3·7	Rj	Early.
126	6044	192	S Herculis	5·9—7·7	11·5—12·2	303	5·6	Rj	Early.
128	6132	195	R Ophiuchi	7·6—8·1	< 12	302·4	4·5	Rj	7
138*a*	6682	—	— Serpentis	6·8	9	300	5	—	—
146	6849	221	R Aquilæ	6·4—7·4	10·9—11·2	345	5·5	Rrj	9
175	7609	—	T Cephei	6·2—6·7	9·5—9·9	390	6·3	—	—
187	8512	298	R Aquarii	5·8—8·5	11	388	4·3	Jr	9

Variables with periods of 400 to 500 days.

Gore.	Chandler.	Dunér.	Name.	Max.	Min.	Period.	Colour.		Species.
							Dunér.	Chandler.	
1	107	3	T Cassiopeiæ	6·5—7·0	11—11·2	436	Rrj	7·3	Early.
2	112	4	R Andromedæ	5·6—8·6	< 12·8	404·7	Rrj	5·0	10
32	1855	—	R Aurigæ	6·5—7·4	9·2—12·7	465	—	6·5	—
34	1944	—	S Orionis	8·3—9	< 12·3	413	—	6·4	—
89	4826	141	R Hydræ	4—5	10?	434±	Rr	5·9	10
119	5889	181	U Herculis	6·6—7·7	11·4—11·6	408·3	Rrj	6·5	7
151	7045	231	R Cygni	5·9—8	13	425·3	Rrj	6·0	Late.
154	7120	239	χ Cygni	4—6	12·8	406·5	Rrj	6·3	10
189	8600	—	R Cassiopeiæ	4·8—6·8		429±	—	6·5	—

III. The Relation of Colour to Period.

Mr. Chandler's Observations.

In the tables given the particulars relating to period and range of variability are taken from Gore, and Chandler's colour-numbers are placed in a separate column.

Mr. Chandler has shown* that there is an intimate connexion between the length of period of a variable star and its colour. In general, the longer the period the redder the tint. If the period is between 500 and 600 days, the mean redness on his scale is about 7·5; for periods of about 300 days, it is about 3; and for shorter periods it is 1 or 2. This is exactly what would happen if my theory were true.

In order to investigate the cause of this relation it is necessary that I should refer to Chandler's work in connexion with my previous classification of the 297 bodies of Group II spectroscopically observed by Dunér.

The Relation of Colour to the Degree of Condensation in Swarms of Group II.

In the Bakerian Lecture I provisionally divided the bodies of Group II into fifteen species, the first being the least and the last the most condensed swarms. If then the degree of condensation of a swarm has any relation to colour, the work of Chandler on the colours of variable stars, taken in conjunction with this classification, ought to enable us to determine the nature of such relation.

In order to determine Chandler's colour-numbers corresponding to these, tables were prepared comparing Dunér's colours of the variable stars of the group with the colours assigned by Chandler to the same stars. Two stars which Dunér gives as Rrrj occur in Chandler's list, the colours being 6·9 and 8·1 respectively, or a mean of 7·5.

The colour-number corresponding to Rrrj has therefore been taken as 7·5. Similarly, there are ten Rrj stars in Dunér's list for which the mean colour-number assigned by Chandler is 5·9, and so on.

Dunér's Colour = Rrrj.

No. (Dunér).	Colour (Chandler).
92	6·9
106	8·1
	Mean 7·5.

* 'Astr. Journ.,' No. 179—180.

Dunér's Colour = Rrj.

No. (Dunér).	Colour (Chandler).
37	4·5
91	6·0
221	5·5
3	7·3
4	5·0
141	5·9
181	6·5
231	6·0
239	6·3
269	6·2
	Mean 5·9.

Dunér's Colour = Rj.

196	5·0
238	3·0
165	3·0
266	5·0
128	3·2
158	3·6
186	3·2
9	2·0
18	5·9
23	3·2
68	4·1
76	5·3
118	3·7
166	4·9
170	3·7
192	5·6
195	4·5
50	6·0
	Mean 4·2.

Dunér's Colour = Jr.

281	2·0
184	3·0
20	2·4
55	3·0
127	1·3
261	2·0
81	2·1

No. (Dunér).	Colour (Chandler).
125	2·0
159	2·7
187	2·0
222	3·6
100	1·6
293	4·3
29	2·0
	Mean 2·4.

Dunér's colour = Jjr.

None common to Dunér and Chandler.

Mean colour, say, 0·7.

It will be seen that the increments for one colour stage of Dunér are 1·6, 1·7, and 1·8 respectively, or a mean of 1·7. Since there are none of Dunér's Jjr stars in Chandler's list, we may use this increment to approximate to the colour; this gives us the number 0·7. We thus get:—

Dunér's colour.	Chandler's number.
Rrrj	7·5
Rrj	5·9
Rj	4·2
Jr	2·4
Jjr	0·7

Using these mean numbers, we may determine the mean colour-number associated with each of the fifteen species into which Group II has been divided. The following tables show the results obtained.

	Dunér's No.	Colour.
Species 2.	56	4·2
	93	4·2
	220	2·4
	223	2·4
	246	2·4
		Mean 3·1.
Species 3.	42	2·4
	53	2·4
	70	2·4
	185	2·4
	198	2·4
	228	4·2
	276	0·6
	290	2·4
		Mean 2·4.

	Dunér's No.	Colour.
Species 4.	7	0·6
	95	2·4
	110	2·4
		Mean 1·8.
Species 5.	89	4·2
	153	2·4
	154	2·4
	173	2·4
	253	4·2
	258	2·4
	267	2·4
	271	2·4
		Mean 2·85.
Species 6.	6	2·4
	19	2·4
	39	2·4
	48	4·2
	67	2·4
	74	2·4
	76	4·2
	83	2·4
	99	2·4
	188	4·2
	189	4·2
	194	2·4
	202	2·4
	208	4·2
	214	4·2
	227	2·4
	247	2·4
	254	4·2
	259	2·4
	260	2·4
	273	4·2
	274	2·4
	285	2·4
	289	0·6
		Mean 2·9.
Species 7.	24	0·6
	97	2·4
	115	2·4
	143	2·4

	Duner's No.	Colour.
	181	5·9
	195	4·2
	229	2·4
	241	4·2
	249	4·2
	252	4·2
	256	4·2
	269	5·9
	270	4·2
	275	4·2
	284	2·4
		Mean 3·6.
Species 8.	15	2·4
	29	2·4
	57	4·2
	88	2·4
	103	2·4
	108	2·4
	112	0·6
	137	4·2
	161	4·2
	166	4·2
	184	2·4
	216	4·2
	225	4·2
	230	2·4
	242	2·4
	251	4·2
	263	2·4
	278	2·4
	283	2·4
	286	2·4
	291	2·4
	295	2·4
	297	2·4
		Mean 2·9.
Species 9.	9	4·2
	12	5·9
	20	2·4
	23	4·2
	25	2·4
	37	5·9
	44	4·2

	Dunér's No.	Colour.
	65	2·4
	66	2·4
	118	4·2
	123	5·9
	148	2·4
	156	5·9
	158	4·2
	162	4·2
	165	4·2
	174	4·2
	175	4·2
	176	4·2
	183	4·2
	186	4·2
	204	4·2
	217	4·2
	221	5·9
	237	2·4
	255	2·4
	266	4·2
	277	2·4
	281	2·4
	293	2·4
		Mean 3·9.
Species 10.	4	5·9
	18	4·2
	28	4·2
	30	4·2
	86	4·2
	91	5·9
	92	7·5
	131	2·4
	141	5·9
	172	5·9
	196	4·2
	232	4·2
	239	5·9
		Mean 5.
Species 11.	5	2·4
	55	2·4
	87	4·2
	98	2·4
	135	2·4

	Dunér's No.	Colour.
	149	4·2
	152	2·4
	171	2·4
	177	2·4
	191	2·4
	193	2·4
	197	2·4
	199	2·4
	212	2·4
	218	4·2
	234	2·4
	245	2·4
	288	2·4
		Mean 2·7.
Species 12.	27	2·4
	46	2·4
	51	2·4
	52	2·4
	60	2·4
	78	4·2
	117	2·4
	122	0·6
	126	2·4
	129	2·4
	133	2·4
	164	4·2
	215	2·4
	264	2·4
		Mean 2·5.
Species 13.	1	2·4
	2	4·2
	16	0·6
	17	2·4
	26	0·6
	32	2·4
	33	2·4
	36	2·4
	38	4·2
	40	2·4
	54	4·2
	61	2·4
	62	2·4

Dunér's No.	Colour.
64	2·4
69	2·4
71	2·4
75	2·4
82	0·6
104	2·4
109	2·4
116	2·4
120	2·4
121	2·4
124	2·4
130	2·4
132	2·4
144	2·4
145	2·4
146	2·4
155	2·4
160	2·4
182	4·2
200	0·6
203	2·4
205	2·4
207	2·4
211	2·4
240	2·4
243	2·4
244	2·4
268	2·4
280	2·4
2[illegible]7	2·4
292	2·4
294	2·4
	Mean 2·4.
Species 14. 22	2·4
49	2·4
90	2·4
94	2·4
107	2·4
111	4·2
113	2·4
140	2·4
142	2·4
167	2·4

Dunér's No.	Colour.
169	2·4
179	2·4
180	2·4
187	2·4
250	4·2
282	0·6
138	2·4
	Mean 2·5.

Species 15.	41	2·4
	50	4·2
	96	2·4
	101	2·4
	136	4·2
	139	2·4
	147	0·6

Species 15.	190	4·2
	226	2·4
	235	2·4
	265	2·4
	279	2·4
		Mean 2·7.

We thus get the following colour-numbers corresponding to the fifteen species:—

Species.	Mean colour-number.
1 (1 star) (?)	4·2
2 (5 stars)	3·1
3 (8 „)	2·4
4 (3 „)	1·8
5 (8 „)	2·85
6 (24 „)	2·9
7 (15 „)	3·6
8 (23 „)	2·9
9 (30 „)	3·9
10 (13 „)	5·0
11 (18 „)	2·7
12 (14 „)	2·5
13 (45 „)	2·4
14 (17 „)	2·5
15 (12 „)	2·7

The remaining stars observed by Dunér are not included in the classification at present, owing to insufficient details.

The result of this comparison of Dunér's and Chandler's observations, taken in conjunction with my classification in species of the bodies of Group II, goes to show that the swarms with a mean condensation are the reddest. For, although the results are not quite so uniform as might be desired, there is a decided maximum of redness in species 9 and 10, which may fairly be taken as the swarms with mean spacing. The greatest discrepancy is in Species 1, but here the result depends upon the observations of one star, and even that is not definitely known to belong to Species 1. (See "Bakerian Lecture," p. 65.)

It may be objected that the foregoing series of numbers is not sufficiently regular for any trustworthy conclusions to be arrived at. But the very decided maximum in Species 10 is of itself sufficient evidence that the irregularities on both sides of it are due to the difficulties of observation. I have gone over Dunér's observations of the spectra and colours of the bodies of Group II without reference to my temperature classification, and the result shows that where the spectra are described as identical, the colours sometimes differ considerably. The table on page 419 shows that this is the case. The numbers in the vertical columns indicate the numbers of stars of any particular colour associated with a particular spectrum. Thus, amongst the stars with a spectrum containing the band 1—10 uniformly developed, 3 have the colour Rrj, and 5 are Rj.

It will be seen, therefore, that, even if my classification into species be not accepted, the relation between colour and spectrum in the present state of our knowledge is not absolutely definite.

[This is probably to a great extent due to the variability of the stars of the group. All of them may be more or less variable, and it may often have happened that the colour of a star has been recorded at one time and its spectrum at another, when the colour was slightly different. Some of the slight variations observed may also be due to variation in the atmospheric absorption.—November 1, 1889.]

On reference to the tables of variables which I give in this paper it will be seen also that the relation between colour and period observed by Chandler is only a general one.

We may, therefore, for the present regard the swarms with mean spacing as the reddest. The sparsest swarms vary from blue to greenish-white, so that the redness will gradually deepen in passing from these to the mean swarms. Again, in passing from the mean swarms to the most condensed ones, the redness must gradually disappear, for we know that the stars of Group III are yellow or white.

The following represents the colour-condition of stars of Group II both more and less condensed than the mean swarms.

Spectra.	Rrj.	Rj.	Jr.	Jjr.
Bands narrow and pale, red stronger	—	2	10	—
2—8 bands narrow and pale	—	—	15	—
Bands wide and pale	3	5	9	—
2—8 bands moderately wide and dark, 2 and 3 strong	—	5	36	4
Bands wide and dark, red strongest	—	—	3	2
2—9 bands moderately wide and dark, 2 and 3 strongest	—	4	6	—
1—9 bands moderately wide and dark, 2 and 3 strongest	—	1	2	—
1—10 bands wide and moderately dark, red strongest	—	—	1	—
1—10 bands well developed and equal	3	5	—	—
1—9 blue bands most strongly developed	—	1	1	—
1—9 wide and dark	—	5	4	—
2—9 wide and dark, blue strongest	1	5	4	1
2—9 wide and dark	1	5	21	—
2—10 wide and dark	—	3	—	—
Bands wide and dark, blue strongest	4	13	5	—
2—8 wide and dark, blue strongest	—	1	—	1
Bands wide and dark	5	10	6	1
2—8 wide and dark	—	6	20	1
2—8 narrow and dark	—	1	1	—
Bands narrow and pale, blue strongest	—	—	5	—
2, 3, 4, 5, 7 and 8, 7 and 8 strongest	—	2	6	—
2, 3, 5, 7, 8	—	—	2	1
2, 3, 7, and 8	—	1	6	1
2, 3, 7	—	3	3	—
Indeterminate	3	8	10	—
	20	86	176	12

Group II.... { reddish-yellow, yellowish-red, red, yellowish-red, reddish-yellow.

Hence no definite conclusion as to temperature of Group II stars can be arrived at by colour observations alone, since stars cooler than the mean, as well as hotter, give the same colour.

The Cause of the Relation between Colour and Period.

On reference to the tables of variables, it will be seen that there are none less condensed than Species 7. This means that the sparsest swarms either exhibit no variability at all, or their variability is of such a character as to escape notice. The reason for this is not far to seek. Firstly, if there be any revolving swarms with small orbits, they will never be entirely out of the central swarm, and their effect will simply be to produce a general increase of brightness of the swarm.

Only revolving swarms with large orbits will therefore be effective in producing variability, but even these will only cause variability of short range, since the number of collisions at periastron passage will be small, the swarm being sparse. In the sparsest swarms, therefore, the variability will be of a long period and the range will be small. These are no doubt the causes of the variability having been overlooked.

When we pass to the mean swarms, however, the variability becomes more strongly marked. Cometic swarms of short period, if they exist at all, will still only produce a general brightening of the central swarm, and the swarms most effective in producing variability will therefore be those with moderately long periods. The range of variability will depend upon the eccentricity of orbit and the periastron distance of the revolving swarm, as in the general case.

As the central swarm becomes more and more condensed, and therefore gradually loses its redness, only shorter period swarms will be effective in producing variability, as the outliers will have been drawn entirely within the orbits of longer period swarms, if they exist at all.

Still further condensation of the central swarm will draw the outliers within the orbits of the revolving swarms, which would produce variability in the swarms last considered, and now only very short period swarms are concerned. At the same time the colour will have become yellow or yellowish-white, the swarm having passed from Group II to Group III.

It will be seen that my theory perfectly explains the general relation of period to colour which has been observed by Chandler and previously by Schmidt,* and in fact demands it.

The range of variability does not appear to bear any relation to the periodicity (except perhaps in the sparsest swarms), and this is only what we should expect, as the conditions on which the range depends (periastron distance, and eccentricity of orbit of revolving swarm) are special to each star. Cometic swarms in our own system follow no general rule as regards the eccentricities of their orbits, or their perihelion distances.

IV. The Irregular Variables of Group II.

The next test is that of irregularity. The apparent irregularities in the variability of stars in the group under discussion are, on my theory, produced by the revolution of several swarms of meteorites at different rates and different distances round the central one. The swarms most subject to irregularity should, therefore, on this view, be those which are most likely to suffer from the effects of the

* Quoted in 'Observatory,' Feb., 1889.

greatest number of revolving swarms. These will not be the sparsest swarms, for the reason that the short period swarms will only produce a general brightening, as already pointed out, leaving the long period swarms predominant. Neither will they be the most condensed, because most of the cometic swarms will sweep clear of the central swarm at periastron passage. They must, therefore, occur in the swarms of mean condensation, if anywhere at all, though mean swarms need not necessarily exhibit irregular variability. The facts observed show that out of the five irregular variables of Group II, three have colours indicating a mean condensation, while two appear to be a little further condensed.

Irregular Variables.

Gore	Chandler.	Dunér.	Name.	Maximum.	Minimum.	Period.	Colour.		Species.
							Chandler.	Dunér.	
18	1072	29	ρ-Persei	3·4–	4·2	—	2	Jr	8
37	2098	50	α-Orionis	1	1·4	—	6	Rj	15
129	6181	196	α-Herculis	3·1	3·9	—	5	Rj	10
179	7803	269	μ-Cephei	2·7	4·8	—	6·2	Rrj	7
184	8273	281	β-Pegasi	2 2	2·7	—	2	Jr	9

The spectroscopic observations confirm the conclusion that irregularity mostly occurs in mean swarms; it will be seen that with the exception of α Orionis, which is only very slightly variable, the species to which the irregular variables belong are 7—10, indicating mean condensation.

V. Bright Hydrogen in Variable Stars of Group II.

I have already pointed out* that in the class of variable stars under consideration the bright lines of hydrogen might be expected to make their appearance at maximum. For since the bodies of Group II are very much akin to nebulæ, an increase of temperature such as occurs at maximum should be accompanied by the appearance of bright hydrogen, because a greater quantity of incandescent gas would then occupy the interspaces.

Under normal conditions there are neither bright nor dark hydrogen lines in the spectra of bodies of Group II, the simple and sufficient explanation being that the bright lines from the interspaces balance the dark lines from the meteoritic nuclei. "Anything which in this condition of light-equilibrium will increase the amount of incan-

* Bakerian Lecture, 1888, p. 83.

descent gas and vapour in the interspaces will bring about the appearance of the hydrogen lines as bright ones. The thing above all things most capable of doing this in a most transcendental fashion is the invasion of one part of the swarm by another moving with a high velocity. This is exactly what I postulate. The wonderful thing under these circumstances then would be that bright hydrogen should *not* add itself to the bright carbon, not only in bright line stars, but in those the spectra of which consist of mixed flutings, bright carbon representing the radiation."*

That the bright lines of hydrogen do make their appearance at maximum, in some of the stars at all events, is placed beyond doubt by the recent observations of Mr. Espin at Wolsingham.

On August 13, 1888, Mr. Espin† noted "a very bright line, apparently F," in the spectrum of R Cygni, the maximum of the star occurring on July 19th.

The spectrum of *o* Ceti was also observed by Mr. Espin† on October 23rd and 30th, 1888, the maximum of the star occurring on September 28th. Dunér's bands from 1 to 10 were seen, and the observer noted that on October 30th, when the star had faded considerably, bands 8, 9, and 10 seemed to be broken into two, but he was doubtful whether these interferences were due to bright lines or not. A brilliant line was observed in the violet, which was thought to be *h* (hydrogen). It is very probable also that bright F was present on this date and caused the second maximum in band No. 9.

Bright lines of hydrogen and other substances were photographed in the spectrum of Mira by Professor Pickering in November, 1886, the maximum occurring on November 14th.

Mr. Maunder‡ observed bright hydrogen (G) in the spectrum of Mira on October 5th, 1888, but on December 1st it was not recorded.

Mr. Espin has also announced in a recent circular (April 2nd, 1889) that there are bright lines in the spectra of R Leonis and R Hydræ. He states that "the spectra of R Leonis and R Hydræ contain bright (hydrogen?) lines, first seen on February 25th. Observations confirmed, through the kindness of Mr. Common, by Mr. Taylor, at Ealing, who sees two in R Leonis and one in R Hydræ." Both these stars were near their maxima at the time of observation, that of R Leonis occurring on March 23rd, and that of R Hydræ on February 17th.

[Another circular (October 3, 1889) states that "Bright lines were seen in the spectrum of R Andromedæ on September 25th, the F line being very bright." The maximum occurred on July 25th.—November 1, 1889.]

* Bakerian Lecture, p. 83.

† 'Ast. Soc. Monthly Notices,' vol. 49, p. 18.

‡ 'Ast. Soc. Monthly Notices,' vol. 49, p. 304.

The appearance of the hydrogen lines at the maximum and their disappearance as the stars fade will no doubt eventually be found to be among the characteristic variations of the spectrum which accompanies the variation of light in stars of this class.

VI. Conclusion.

As far as Group II is concerned, I think it will be granted that the meteoritic theory of variability is quite in harmony with the facts observed, considering that the observations are still incomplete. The theory does not require that all the swarms of the group should be variable, but only those which are condensing double or multiple nebulæ. At the same time it requires that this group should be more subject to variability than any of the others, and this is one of the best established facts of modern astronomy. Not only are these general demands satisfied, but the theory bears the strain put upon it when it is used to explain the finer details, as I have shown in this paper.

III. "On the Local Paralysis of Peripheral Ganglia, and on the Connexion of different Classes of Nerve Fibres with them." By J. N. Langley, F.R.S., Fellow of Trinity College, and W. Lee Dickinson, M.R.C.P., Caius College, Cambridge. Received September 7, 1889.

Hirschmann* has shown that after a moderate dose of nicotin stimulation of the sympathetic nerve in the neck causes no dilation of the pupil. He concludes that nicotin paralyses the endings of the dilator fibres in the pupil.

In the course of some observations on the physiological action of nicotin, we had occasion to repeat Hirschmann's experiment; we found in the rabbit that 30 to 40 mgrms. of nicotin injected into a vein stopped the effect of stimulating the sympathetic in the neck, not only on the pupil, but also on the vessels of the ear. A paralysis of the vasomotor fibres of the sympathetic had been suggested by Rosenthal,† on the ground that nicotin causes a state of congestion in the vessels of the ear of the rabbit.

Since we had been much struck with the profound action of nicotin upon the central nervous system, and since it had seemed to one‡ of us in some previous experiments with atropin that the secretion of saliva from the sub-maxillary gland of the cat failed earlier on stimu-

* Hirschmann, 'Arch. f. Anat. u. Physiol.,' 1863, p. 309.

† Rosenthal, 'Centralb. f. d. Med. Wissenschaften,' 1863, p. 737.

‡ Langley, 'Journal of Physiology,' vol. 1, 1878, p. 89.

lation of the sympathetic nerve in the neck than on stimulation of the sympathetic fibres proceeding from the superior cervical ganglion, it occurred to us that the action of nicotin might be due to a paralysis of the nerve cells of the superior cervical ganglion, and not to a paralysis of the peripheral endings of the sympathetic nerve. On testing this view, by stimulating the sympathetic above and below the superior cervical ganglion after injection of nicotin, we found that, whilst stimulation below the ganglion produced no effect, stimulation above the ganglion produced a dilation of the pupil and a constriction of the vessels of the ear, as if no nicotin had been given. The effect of stimulating the nerve fibres above the ganglion is not abolished by an amount of nicotin four to five times as great as that sufficient to abolish the effect of stimulating the sympathetic nerve in the neck. This point, however, we shall consider more in detail in a later paper upon the general action of nicotin. We are here only concerned with the fact *that after a certain dose of nicotin stimulation of the sympathetic fibres below the ganglion does not produce dilation of the pupil or constriction of the vessels of the ear, whilst stimulation of the sympathetic nerve fibres above the ganglion produces these changes in the normal manner.*

It is conceivable that the difference in the effect of stimulating above and below the ganglion might be due to the nerve fibres being medullated below and non-medullated above the ganglion, and to nicotin paralysing the former and not the latter. But, in the first place, although it is probable, it has not been shown, that the dilator fibres of the pupil and the vaso-constrictor fibres for the ear are medullated below the ganglion; and, in the second place, it is obvious that medullated fibres as such are not paralysed by nicotin, since for some time after the stage in which stimulation of the sympathetic in the neck fails to affect the pupil or the ear, stimulation of a nerve such as the sciatic will cause movement both directly and reflexly, that is to say, at this stage neither the medullated sensory fibres nor the medullated motor fibres to skeletal muscle are paralysed.

The method of action of nicotin can be tested in a more direct manner. If the alkaloid produces its effect by acting upon the nerve below the ganglion in consequence of any peculiarity of structure obtaining there, the local application of nicotin to the nerve should abolish its irritability. If, on the other hand, it produces its effect by acting upon the nerve cells in the superior cervical ganglion, the local application of nicotin to the nerve should have very little effect upon the nerve irritability, but the local application to the ganglion should abolish the effect of stimulating the nerve centrally of the ganglion.

In making the experiment on these lines, we isolate the sympathetic nerve in the neck, the superior cervical ganglion, and to a certain

extent the filaments proceeding from it to the external and internal carotid arteries. Having stimulated the sympathetic in the neck, and observed its normal action on the eye and on the ear, an inch and a half or so of the nerve is brushed over with a 1 per cent. solution of nicotin. Any excess of fluid around the nerve is removed by blotting paper, and the moistening the nerve with dilute nicotin is repeated. The central part of the nerve is stimulated several times at intervals of about two minutes; it produces the usual dilation of the pupil and constriction of the vessels of the ear. The ganglion and the filaments proceeding from it are then brushed over with 1 per cent. nicotin; the sympathetic in the neck is again stimulated; it is found to be completely without effect; stimulation of the filaments running from the ganglion to the arteries produce the normal action. Hence *nicotin paralyses the cells of the superior cervical ganglion.*

Besides the dilator fibres for the pupil and the vaso-constrictor fibres for the ear, the cervical sympathetic contains vaso-motor fibres for the head generally, and secretory fibres for the salivary glands.* On these we have made a few experiments only; but, so far, we find that (in the rabbit and cat) after the application of nicotin to the superior cervical ganglion stimulation of the cervical sympathetic no longer causes secretion or pallor in the sub-maxillary gland, nor pallor of the mouth. In fact, after nicotin has been applied to the ganglion, we have been unable to detect any effect from stimulating the sympathetic in the neck.

We conclude that *the dilator fibres for the pupil, the vaso-constrictor fibres for the ear (probably also those for the head generally), and the secretory fibres for the glands end in the cells of the superior cervical ganglion.*

The paralysis of the cells is produced with remarkable ease; in the rabbit and cat a complete abolition of the effects of stimulating the sympathetic in the neck results from a single "painting" of the superior cervical ganglion with a small brush dipped in 1 per cent. nicotin. The experiment is most easily performed in the rabbit. In the cat the simplest method is to dissect away the connective tissue on the mesial and dorsal side of the ganglion, to pull upwards and laterally the muscles lying by the carotid, and then, without separating the ganglion of the trunk of the vagus from the sympathetic ganglion, to moisten the exposed medio-dorsal surface of the latter with dilute nicotin. Of course, by this method, some nicotin will be almost certainly applied to the ganglion of the trunk of the vagus; we may mention, as showing that the nicotin affects the nerve fibres

* According to Heidenhain ('Pflüger's Archiv,' vol. 5, 1872, p. 316), when about 15 mgrms. of nicotin are injected into the vein of a dog, the sympathetic secretory fibres are for a short time paralysed—presumably this is for stimulation of the cervical sympathetic.

comparatively little, that, if in the above experiment nicotin 1 per cent. be copiously applied to the vagus or to the ganglion trunci vagi, the inhibitory power of the vagus upon respiration is apparently unaffected.

Although in an experiment conducted in the manner just described there is little or no diminution of irritability of the sympathetic nerve on applying 1 per cent. nicotin to it, repeated application of nicotin to the nerve does, as might be expected, lower and finally destroy its irritability. And if the nerve is ligatured and a long piece isolated so that the blood supply to it is cut off, a great reduction or even abolition of irritability takes place on soaking it with 1 per cent. nicotin. But, with proper precautions, the difference in the effect of applying nicotin to the ganglion and to the nerve is so great that there is practically no danger of confusing the action on the cells with that on the nerve fibres. In the nerves of the frog, the effect on the nerve fibres, other things being equal, has seemed to us to be greater than in the mammal. Since nicotin is alkaline, it is possible that a part of its injurious effect may be due to its alkalinity. And in fact, if a 2 per cent. solution of nicotin be neutralised with sulphuric acid, and diluted with water so that it contains 1 per cent. nicotin, its effect both upon nerve fibres and upon ganglion cells is lessened. This is especially the case with nerve fibres. The cervical sympathetic may be left for a minute or two in a pool of 1 per cent. nicotin sulphate,* and still on stimulation cause maximum dilation of the pupil. The superior cervical ganglion requires a freer application of 1 per cent. nicotin sulphate than of 1 per cent. nicotin to paralyse it, but the paralysis is still readily produced. The period of paralysis, after painting the superior cervical ganglion with 1 per cent. nicotin, passes off in twenty to thirty minutes, so that in no long time the sympathetic in the neck produces its usual effects.

To paralyse the ganglion a second time requires a very much larger dose of nicotin than was required the first time. Painting it over with even 0·5 per cent. nicotin without any excess of fluid may be sufficient the first time, but painting the ganglion half-a-dozen times with 1 per cent. nicotin may be required to paralyse it a second time. We had hardly expected to find so marked an example of the habituation to poisons which is known to occur in certain cases, and especially with nicotin. Apparently also the period of paralysis lasts a shorter time after the second application of nicotin than after the first.

As a rule, the application of nicotin to the ganglion causes for a brief period the same effect as stimulating the nerve. The alkaloid appears to excite the nerve cells before paralysing them.

* For convenience we speak of the neutralised solution containing 1 per cent. nicotin as a 1 per cent. nicotin sulphate solution. It contained, of course, a rather higher percentage of the sulphate.

Ganglion of the Solar Plexus.

In the dog, cat, and rabbit the splanchnic nerve on the left side runs to two chief ganglionic masses. Since the upper of these ganglia sends its nerves chiefly to the cœliac axis and the lower sends its nerves chiefly to the superior mesenteric artery, we may call these respectively the cœliac and superior mesenteric ganglia. From the solar plexus nerve fibres run to the kidney. Usually these are joined by fibres direct from the splanchnic. In the cat and dog there has been in the cases we have examined a lesser splanchnic, running partly to the renal plexus and partly to the solar plexus. The renal ganglia are, as is well known, scattered, but in the dog the chief one often lies underneath the supra-renal body, and in the cat the chief one is placed between the artery and vein on fibres proceeding chiefly from the superior mesenteric ganglion and about $\frac{1}{3}$ inch from it.

Our experiments upon the connexion of the splanchnic with the ganglia of the solar and renal plexus have been made almost entirely on the left side, and in the following account we speak of the nerve and ganglia of the left side, unless the right side is especially mentioned.

When the stomach and intestine are exposed there are usually slight movements of the intestines, and there may be movements of the stomach. When these are absent they may be brought about, with a degree of distinctness varying with the animal, by stimulating the vagus. These movements continue for a short time after the nerve stimulation has ceased. Stimulation of the splanchnic stops the movements, whether they are spontaneous or are occurring as the result of previous vagus stimulation. These facts are well known; but whether the inhibitory fibres of the splanchnic end in the nerve cells of the solar plexus has so far been guess work. To determine this we have proceeded as in the case of the superior cervical ganglion. Having ascertained that the application of 1 per cent. nicotin or nicotin sulphate to the splanchnic leaves its inhibitory power unaffected, we have painted one or other of the ganglia, or the whole plexus, with a small brush moistened with 1 per cent. nicotin or nicotin sulphate. Nicotin applied to the whole plexus at once abolishes the inhibitory power of the splanchnic, but inhibition, although naturally much less perfect, can still be produced by stimulating the fibres proceeding from the ganglia. Hence *the inhibitory fibres of the splanchnic end in the cells of the solar plexus.* Further, if the superior mesenteric ganglion be brushed over with nicotin, stimulation of the splanchnic is still able to produce inhibition of the movements of the stomach, but is without any appreciable effect upon the movements of the intestine. On the other hand, when nicotin is applied to the cœliac ganglion, the inhibitory power of the splanchnic upon the intes-

tines is not abolished, but that upon the movements of the stomach in the main at any rate is abolished. Our experiments are not sufficiently numerous, especially with regard to the connexion of the cœliac ganglion with the stomach, to make it certain that the one ganglion is entirely connected with the fibres to the intestine, and the other the fibres to the stomach, but we think they show *that in the main, and possibly altogether, the stomachic inhibitory fibres of the splanchnic nerve end in the cells of the cœliac ganglion, and the intestinal inhibitory fibres of the splanchnic end in the cells of the superior mesenteric ganglion.* The vagus is said to send fibres to the ganglia of the solar plexus. We find, however, that copious application of nicotin to the plexus on both right and left sides of the body does not interfere with the movements of the stomach and intestines produced by stimulating the vagus in the neck: that is to say, *the motor fibres of the vagus do not end in the nerve cells of the solar plexus.*

We may note that after nicotin has been applied to the ganglia of the solar plexus the spontaneous movements of the intestine become more pronounced; that the ganglia recover in twenty to thirty minutes from their state of paralysis; and that to produce paralysis a second time a larger amount of nicotin is required.

The connexion of the vaso-motor fibres of the splanchnic with the nerve cells of the solar plexus can be determined by taking a tracing of the arterial blood pressure and stimulating the splanchnic before and after the application of nicotin to the ganglia. In the rabbit and cat, brushing either the cœliac or the superior mesenteric ganglion with 1 per cent. nicotin sulphate diminishes the effect of stimulating the splanchnic. The rise of blood pressure produced is much less than previous to the application of nicotin. By applying nicotin to both ganglia, being careful not to allow any to reach the renal plexus, the rise of blood pressure caused by stimulating the splanchnic is reduced to very small limits—in the rabbit, indeed, there may be no rise of blood pressure—and, by applying it to the renal plexus as well, the effect of splanchnic stimulation on the blood pressure is abolished.

We have obtained some evidence that, as in the case of the inhibitory splanchnic fibres, so the vaso-motor splanchnic fibres for the area of distribution of the cœliac artery run to the cœliac ganglion, and those for the area of distribution of the superior mesenteric artery run to the superior mesenteric ganglion; but the method of determining this, viz., by observing the state of pallor of the viscera, often gives unsatisfactory results.

Bradford has recently shown that vaso-dilator fibres run in the splanchnics to the kidney, and probably to the stomach and small intestines. We find that after nicotin has been applied to the ganglia of the solar and renal plexuses stimulation of the splanchnics causes

no fall of blood pressure. *We conclude that the vaso-dilator as well as the vaso-constrictor fibres of the splanchnic end in the cells of the solar and renal plexuses.*

The connexion of the renal fibres with nerve cells, although it can to a certain extent be deduced from observations like those we have just given, is most satisfactorily made out by noting directly the volume of the kidney with the aid of Roy's oncometer. We have so far only made this observation on the dog. In the dog copious application of nicotin 1 per cent. to the ganglia of the solar plexus does not prevent stimulation of the splanchnic from causing a normal large constriction of the vessels of the kidney. This constriction, in the few experiments we have made, has been as great as that occurring before the application of nicotin to the solar plexus. On the assumption that the constriction would be less if some of the vaso-constrictor fibres had been put out of action, we conclude *that few if any of the splanchnic vaso-constrictor fibres for the kidney end in the ganglia of the solar plexus.* On separating the supra-renal capsule from the underlying tissue, and applying nicotin to the ganglia which lie underneath its lateral part, a decrease in the effect of splanchnic stimulation occurs, and on brushing nicotin on the artery near the supra-renal capsule there is a still further decrease in the effect. Since the dogs on which we have experimented have had much fatty tissue around the artery and vein, we have not succeeded in laying bare the whole of the renal plexus without some mishap, and to this we attribute the fact that in the dog we have not obtained by the application of nicotin a complete abolition of the vaso-constrictor power of the splanchnic upon the kidney. Combining, however, the oncometer observations on the dog with the blood pressure observations on the rabbit and cat, we think there is fair evidence that the *splanchnic vaso-motor fibres for the kidney end in the cells of the renal plexus.*

The immediate effect of the application of nicotin to the ganglia of the solar plexus is a rise of blood pressure and a dilation of the kidney, followed by a fall of blood pressure and a constriction of the kidney. The application of nicotin to the ganglia of the renal plexus causes a constriction of the kidney followed by a dilation, both being greater than when nicotin is applied to the solar plexus, and with a comparatively small effect on the blood pressure. Whilst normally stimulation of the splanchnic in most cases causes a slight primary dilation of the kidney, corresponding with the rise in blood pressure from constriction of vessels of the stomach or intestine, after nicotin has been given we have in no case observed a primary dilation of the kidney or stimulation of the splanchnic.

We have experimented upon various peripheral ganglia other than those mentioned above, and, though our results are as yet incomplete,

with essentially similar results: that is, we have obtained an abolition of the effect of some one or more of the classes of nerve fibres running to them. We think then there is fair ground to conclude that *by stimulating the nerve fibres running to and those from any peripheral ganglion, before and after the application of dilute nicotin to it, the class of nerve fibres which end in the nerve cells of the ganglion can be distinguished from those which run through the ganglion without being connected with nerve cells.*

There are various other questions suggested by the action of nicotin which we hope to deal with later.—Does the paralysis of the ganglion on the posterior root prevent the passage of a stimulus to the central nervous system? Are all afferent fibres connected with nerve cells in the posterior root, or do some run through the ganglion or end elsewhere? Can centres be isolated, or the connexions of tracts followed in the brain and spinal cord? Does any poison when locally applied to ganglia affect unequally the cells in which the different classes of fibres end?

We append an account of an experiment upon the splánchnic to illustrate the method employed.

Rabbit.—5 mgrms. morphia hydrochlorate injected subcutaneously. Chloroform. Cannula in carotid artery for kymographic tracing. Left splanchnic nerve dissected out for 1¼ inch, ligatured, and cut. Peristalsis good. Secondary coil at 7 cm.; this gives a current fairly strong to tip of tongue. Originally, and as a rule after each application of nicotin sulph. to the nerve or ganglia, the splanchnic was stimulated three times at intervals of a minute: since the effect of each of the three stimulations was the same, we mention below one only.

1.18 P.M. Stim. splanchnic for 30 sec. Blood pressure rose rapidly after 2 sec. stim., in 4 sec. rising from 70 to 90 mm. Hg, where it remained for rest of stim., sinking gradually afterwards and regaining previous level in 1¼ min. Peristalsis of intestines inhibited.

1.21 „ Splanchnic painted nearly up to the solar plexus with nicotin sulph. 1 per cent. The nicotin was freely applied several times, a small piece of sponge being placed under the nerve close to the solar plexus to prevent the alkaloid from reaching the ganglia.

1.26 „ Stim. splanchnic for 15 sec. Blood pressure rose in same manner from 62 to 80 mm. Hg. Peristalsis of intestines inhibited.

1.30 „ Cœliac ganglion painted with nicotin sulph. with a small brush, a portion of superior mesenteric ganglion also being touched.

1.33 „ Stim. splanchnic for 15 sec. Blood pressure rose in same manner from 72 to 80 mm. Hg. Peristalsis of intestines inhibited, but apparently less readily.

1.35 „ Nicotin sulph. applied to whole region of solar plexus, no excess of fluid being used.

1.40 „ Stim. splanchnic for 45 sec. Blood pressure remained at same level, 68 mm. Hg. Peristalsis of intestines not inhibited.

2.5½ „ Stim. splanchnic for 15 sec. Blood pressure rose in same manner as at first, rapidly from 58 to 79, and subsequently to 86 mm. Hg. Peristalsis of intestines inhibited. Thus, in half an hour the paralysis of the ganglia had disappeared.

2.6 P.M. Whole region of solar plexus painted with nicotin sulph.

2.10 " Stim. splanchnic for 45 sec. Blood pressure remained at same level, 60 mm. Hg. In the tracing there were no respiratory variations, but at intervals of 25 to 30 sec. there was a slight fall of the blood pressure.

IV. "On the Tubercles on the Roots of Leguminous Plants, with special reference to the Pea and the Bean." By H. MARSHALL WARD, M.A., F.R.S., F.L.S., late Fellow of Christ's College, Cambridge, Professor of Botany in the Forestry School, Royal Indian Engineering College, Cooper's Hill. Received October 22, 1889.

(Preliminary Paper.)

In the 'Philosophical Transactions' for 1887 (vol. 178, B, pp. 539—562, Pl. 32 and 33) I published the results of some investigations into the structure and nature of the tubercular swellings on the roots of *Vicia faba*, the broad-bean of our gardens, paying attention to the bearing of the facts on other Leguminous plants, and discussing what had been done and written at various times concerning these curious structures.

The chief facts established in that paper were as follows:—That the tubercles occur in all places and at all times on the roots of Papilionaceous plants growing in the open land, but that in sterilised media and in properly conducted water-cultures they are not developed, unless the root is previously infected by contact with the contents of other tubercles. In other words, the tubercles can be produced at will by artificial infection. I also showed that the act of infection is a perfectly definite one, and is due to the entrance into the root-hair of a hypha-like infecting tube or filament, which starts from a mere brilliant dot at the side or apex of the root-hair, passes down the cavity of the latter, traverses the cortex of the root from cell to cell, until its tip reaches the innermost cells of the cortex, where it branches and stimulates these cells to divide and form the young tubercle.

It should be noted that these fruits of the infection were entirely new, as were the methods, and that I showed actual preparations of the infecting filaments passing down the root-hairs, to several botanists at the time (June, 1887).

In my paper were also explanations of several points hitherto obscure—such as the curious trumpet-shaped enlargements of the filaments where they transverse the cell-walls of the tissues, suggesting that they were due to subsequent stretching of the walls of the meristematic cells. Also the peculiar haustorium-like swellings of

intra-cellular filaments, and the minute "bacteroids" (which I termed "gemmules") were described. Again, I called attention to the remarkable coiling and distortion of the root-hairs at the point of origin and entry of the infecting filament. It may be recalled to mind that I wrote of the minute brilliant dot at this spot "unless this dot is one of the above-named 'germs' (*i.e.*, a 'bacteroid' or 'gemmule') I do not know what it can be" (p. 548). I also distinctly pointed out that the twisting of the root-hair at the point of infection might be due to the wall of the root-hair growing elsewhere, but not at that point. As to the "gemmules" or "bacteroids"* with which the cells of the inner parts of the tubercle are filled, and their relations to the filaments, I expressed myself (somewhat cautiously it is true) to the following effect. From their curious shapes—those of the letters V, Y, and X, and even more branched figures—I suggested that these bodies propagate by budding; and from their relations to the swollen ends of the intra-cellular branches of the filaments it was not improbable that they are budded off from these, and multiply by further budding in the protoplasm, &c., of the cells. Owing to the extreme minuteness and high refractive index of these bodies, however, I could not definitely decide as to the method of propagation; although no doubt existed that they are living "germs," on the one hand, and that they originate from the filaments, on the other. I also pointed out that their presence in the protoplasm of the cell stimulates the latter and makes it resemble a plasmodium (p. 547). Other points of importance will be recalled as we proceed.

I have now to draw attention to some results of my further researches into this confessedly difficult subject.

After numerous culture experiments and observations made last year (1888), I have decided to abandon the broad-bean as the subject for histological analysis, chiefly because it takes so long to exhaust its stores of reserve materials; it was better for the cultures to be made with the pea, the cotyledons of which are so much smaller, and the plant of which is more easily managed in every way in water- and pot-cultures. On the other hand, the tubercles and their contents present no essential features of difference, and, indeed, I may say at the outset, that all that has been described with respect to the tubercles of the one is essentially true for those of the other. The position on the roots and the sizes and shapes of the tubercles are the same, and they appear under the same conditions. The colour and general structure of their internal tissues are similar, and the bacteroids of the pea are so little different from those of the bean that it is difficult to believe them specifically distinct.

But I can offer more conclusive evidence than the above for the

* Tschirch's word "bacteroids" is a very convenient one, as it does not commit us to any statement as to the nature of these bodies.

identity of the bacteroids in the two cases. In some of the cultures made in the summer of 1888 I infected the roots of the pea with bacteroids taken from the tubercles of the bean, and as this is a point of some importance, in view of the belief that each species of Leguminosæ may have its own species of bacteroid, I may say a few words on this phenomenon.

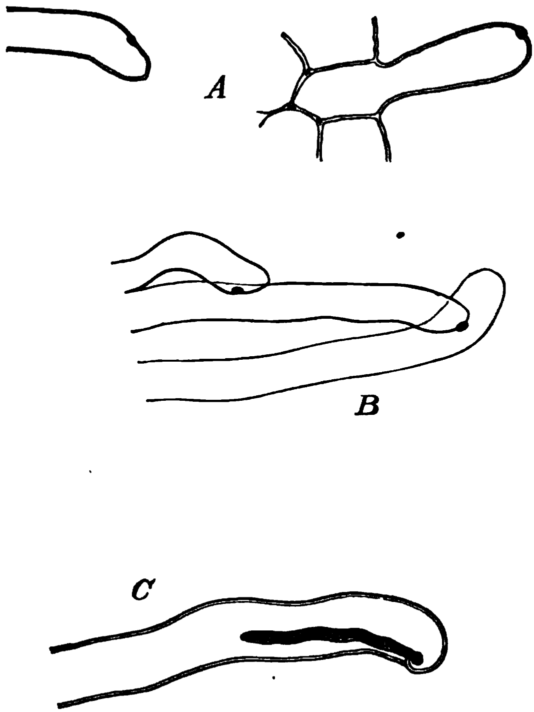
Having satisfied myself that when the contents of the tubercles of the bean are placed among the root-hairs of the young bean root, the latter become infected, I prepared a number of beans and peas as follows: They were allowed to germinate in the sterilised sand until the radicle was about half an inch long; each seedling was then pinned to a cork, and so fixed that the radicle pointed downwards into a large wide-mouthed bottle, in which the cork fitted. The bottle was then carefully lined with filter-paper, kept moist by distilled water; the cork fitted closely, and thus the atmosphere in the bottle was sufficiently damp to enable the radicle to go on growing at the expense of the reserve materials in the cotyledons, and in course of time to put forth a dense pile of delicate root-hairs. As each seedling was pinned on to the cork, I sprayed on to the surface of the radicle, by means of a freshly-drawn capillary tube, a mixture of bacteroids and water made as follows: Tubercles of the bean-roots were carefully washed, placed in alcohol for a few minutes, and then fired, then again washed with distilled water, and pounded in a mortar with distilled water: small drops of this were placed on the radicles as said—*i.e.*, on the radicles of both peas and beans.

In other cases I employed the hanging drops in which I was attempting to cultivate the bacteroids. These consisted of nutritive solutions with asparagin, and with or without gelatine, and in which were placed a few of the bacteroids obtained from cleaned tubercles (cut with a razor sterilised by heat) by means of sterilised needles. I may here say that these cultures (*i.e.*, as micro-cultures) have given me much trouble, and little results: to obtain pure cultures is a matter of greater difficulty than Beyerinck's paper would lead one to expect, and it is not proposed at present to lay much stress on the evidence got from them. Nevertheless, colonies are obtained, and in some cases at least I have transferred the infecting organism from these cultures to the root-hairs of peas and beans.

In any case, I have succeeded in obtaining extracts of the tubercles which contain the infecting germs, and although the latter were always taken from the tubercles of the bean, they infected the root-hairs of both peas and beans equally well.

It is especially the very young root-hairs, with extremely delicate cell-walls, that are infected, and the first sign is the appearance of a very brilliant colourless spot *in* the substance of the cell-wall (figs. A and B): sometimes it is common to two cell-walls of root-hairs in

contact, and not unfrequently one finds several root-hairs all fastened together at the common point of infection (fig. B). This highly refringent spot is obviously the "bright spot" referred to in my pre-



Root-hairs of the pea in process of being infected. A, two very young hairs with the germ in the cellulose wall; B, three root-hairs in the same condition. In C the infecting tube has commenced to grow down the root-hair. The latter is distorted at the point of origin of the tube. The beginning of the distortion is apparent in A (the left-hand figure). A and C = Zeiss J. imm.; B = E obj.

vious paper as the point of infection from which the infecting filament takes origin. It soon grows larger, and develops a long tubular process (fig. C), which grows down inside the root-hair, and invades the cortex, passing across from cell to cell, as described in 1887.

As a matter of fact, then, the "bright spot" is the point of origin of the infecting filament; and, as a matter of inference from the experiments, it cannot but be developed from one of the "bacteroids" or "gemmules" of the tubercles. This attaches itself to the root-hair, fuses with and pierces the delicate cellulose wall, and grows out

into a hypha-like filament at the expense of the cell-contents. The further progress of this filament has already been described in my memoir in the 'Philosophical Transactions' for 1887.

Before proceeding further, reference may be made to some researches made during 1888 and 1889 with the object of learning more about the conditions which rule the development of the tubercles, and the relations of the organism to them. At first I set myself the task of trying to discover a definite spore-stage, thinking that in the rotting tubercles, or at the period of maturity of the plant, or at some other time, it might be that the parasite would betray itself and develop spores: this is not the case apparently, for my experiments seem to prove conclusively that the well-being of the organism of the tubercle and that of the pea or bean go hand in hand. This of course is only so much evidence in favour of the view that we have here a case of symbiosis of the closest kind, as expressed in my previous memoir.

One remark is necessary here. My object throughout had been more especially to determine the nature of the tubercles and of the organism which infests them: the further and larger question as to their function or influence in the economy of the Leguminous plant has been kept subordinate for the present, because I am convinced that more time and appliances are necessary for its complete solution than are at my disposal at present. At the same time some of the following results ought to help in solving the problem as to the possible relations of the tubercle organism to the acquirement of nitrogen by the higher plant.

During the spring and summer of 1888 I made numerous experiments with water-cultures with beans, allowed to germinate in soil so as to be infected by the "germs" therein as demonstrated previously. Several dozens of such cultures were made, and some of them placed in the dark, others in the ordinary light of the laboratory, and some in a well-lighted greenhouse. Tables were prepared showing the number of leaves, living and dead, the condition of the roots, the height of the stem, and so forth, as recorded every week or so (or at shorter intervals) when I examined the plants. It resulted that when the beans are in any way so interfered with that they do not assimilate more material than is necessary for the growth and immediate requirements of the plant, the infecting organism either gains no hold at all on the roots, or it forms only small tubercles which are found to be very poor in "bacteroids:" in some cases the starving plants began to develop tubercles, which never became larger, and in which the infecting organism seemed to be in abeyance. Whether this is due to the bacteroids being developed in small quantities, or to their absorption into the plant is still a question.

I hardened many of these tubercles in picric acid, stained them,

and cut sections, comparing the results with similarly prepared normal tubercles: the chief difference was the paucity in bacteroids, and the prominence of the branched filaments in the cells. Similar results were obtained by placing a box over beans growing in the garden, and comparing the tubercles of the etiolated plants with those of normal plants beside them.

In the spring of this year (1889) I started a series of water-cultures of beans, infected artificially by placing the contents of tubercles on their root-hairs, and kept the roots oxygenated by passing a stream of air through the culture liquid for 24 hours at intervals of a few days: here again the increased growth of the plants—not compensated by increased assimilation—seemed to cause the suppression of the tubercles, or the formation of very poor ones only. These experiments, carried out on several dozens of plants, lead me to conclude that the organism which induces the development of the tubercles is so closely adapted to its conditions that comparatively slight disturbances of the conditions of symbiosis affect its well-being: it is so dependent on the roots of the Leguminosæ, that anything which affects their well-being affects it also.

Some experiments with peas, which are now being tabulated, may throw some light on the wider question which has been raised of late, as to the alleged connection between the development of these tubercles and the increase of nitrogen in Leguminous plants. Thirty-two peas were sown in separate pots of silver-sand, or soil, in five batches of six each, and one of two, and treated in various ways. Six were in garden soil; six in silver-sand, with culture salts, including a nitrate; six in the same medium without nitrate; six in the sand, with traces of soil washings or with pieces of tubercles added; six in sand sterilised by heating; and two in sterilised sand, to which salts (including nitrogen) were added. All but those in the thoroughly sterilised medium bore crops; and these crops have been analysed for me by Professor Green. The soils, water, and other parts are being analysed by Dr. Matthews of Cooper's Hill. I have to thank these gentlemen for the great care and trouble they have kindly taken in this matter.

My object was to decide, if possible, certain points as to the effects of such treatment on the development of the tubercles; but the experiment may possibly turn out more instructive than was at first thought, and will at any rate suggest a line of inquiry to be followed out in the coming spring and summer.

With respect to these plants, I may say that I shall have data showing how much nitrogen was present in the medium at the beginning of the experiment; how much was added; how much the average seedling pea contained; and how much the crop contained in each case. The condition of each plant at convenient intervals was

recorded; the number of living leaves and of dead ones; the height of the stem; number of buds, flowers, and fruits, open and set; and the number of seeds ripened.

The tubercles were developed on all but one of the plants, except those in the completely sterilised media. However, I do not propose to go further into these matters at present, simply contenting myself with pointing out that all the evidence at present goes to show that *the Leguminous plant gains nitrogen by absorbing the nitrogenous substance of the bacteroids from the tubercles;* that nitrogenous substances are thus brought by the "bacteroids" ("gemmules") of the infecting organism of the plant; and that, finally, no satisfactory explanation seems forthcoming as to how the organism obtains this nitrogen in certain cases where no compounds of nitrogen have been added. At any rate, if we regard the pot of sand and its pea as one system, there is in some cases *a distinct gain of nitrogen in the crop, and in the sand at its roots.*

Since the publication of my paper in 1887, several observers have attacked the subject from various points of view. The most important papers are those of Vuillemin,* Beyerinck,† and Praźmowski,‡ who deal with the histology and biology of the subject; and those of Hellriegel and Wilfarth, and of Lawes and Gilbert,§ who have concerned themselves especially with the nitrogen question; papers on accessory matters by Frank, Van Tieghem, and a few others have also appeared in the interval. I propose to deal at present only with Praźmowski's papers, since there are several points in them that have special reference to my work on this subject.

In the 'Botanisches Centralblatt' for 1888,‖ appeared a paper by Praźmowski, on the "Root-tubercles of the Leguminosæ." After shortly summarising the literature, and the various views promulgated by different investigators as to the nature of the swellings, the author proceeds to give an account of his own researches. He gives me the credit of having taken a new departure; speaking critically of the want of experimental proofs for their speculative views on the part of previous observers, he says, "*Eine rühmliche Ausnahme bildet in dieser Beziehung* Marshall Ward, *welcher zuerst in den Weg des physiologischen Experimentes betreten hat*" (p. 217), though he does not regard my methods as complete.

* "Les Tubercules Radicaux des Légumineuses," 'Annales de la Science Agronomique française et étrangère,' vol. 1, 1888.

† 'Botanische Zeitung,' 1888, No. 46, p. 725.

‡ 'Botanisches Centralblatt,' 1888, No. 46, pp. 215—285; and 1889, No. 38, p. 356.

§ 'Phil. Trans.,' vol. 180, 1889, B, pp. 1—107.

‖ No. 46, pp. 215—285. The paper is an abstract of an address to the Polish Naturalists' Congress, July, 1888.

Experiments with peas and kidney beans enabled him to confirm decisively the discovery that plants in sterilised media, watered with boiled water, develop no tubercles on their roots, whereas those in ordinary soil, or in sterilised media to which infective matter from open soil or from tubercles was added, always formed them. In other words, the tubercles arise by infection from without, as I had demonstrated. He then proved that very young tubercles still show the infecting filaments passing down the root-hairs, "*gewöhnlichen Pilzhyphen nicht unähnliche Fäden, welche, Wurzelhaare und Epidermis durchwachsend, in das subepidermale Gewebe der Wurzel eindringen. Diese Fäden hat schon* Marshall Ward *in den Wurzelhaaren der Bohne* (Vicia faba) *beobachtet und auf Grund dieser Beobachtung behauptet, dass die fraglichen Knöllchenorganismen durch Wurzelhaare in die Wurzel eindringen.*" He describes the appearance of the filaments, their bright look, apparent want of membrane at first, granular contents, &c., all in accordance with my statements in 1887.

The granular contents gave him much concern; they are seen as minute rodlets under certain reactions. The branching, piercing of cell-walls, &c., are described as by myself. The only difference here is that Prażmowski believes the rodlets to be the same as the bacteroids.

He completely confirms my observation that the tubercle arises from disturbances produced in the deeper cortical tissues by the infecting filament, and describes the cell-contents, nucleus, &c., so well known. He also points out that in the very young conditions, the bacteroids are simple rodlets, even in cases where they become V, Y, X, &c., shaped later.

To sum up, Prażmowski's account of the whole matter confirms that which I gave to the Royal Society in 1887, excepting that he interprets the origin and nature of the bacteroids differently; he regards them as produced from the *contents* of the filaments—as germ-like bodies developed in the interior of the filaments, and not budded off from them. This is hypothesis only, however, for the author expressly states (p. 253), "*Direct habe ich ihre Theilungen nicht gesehen, obgleich ich mir die Mühe gab, sie in den verschiedensten Nährmedien und unter den verschiedensten äusseren Bedingungen zu züchten.*" He concludes they can only multiply in the still *living* protoplasm.

As to the shapes of the bacteroids and tubercles, Prażmowski's statements agree with those of previous observers, and he also remarks the plasmodium-like appearance of the cell protoplasm at certain stages, as noticed by myself. Some observations on a possible spore formation need not be dwelt upon, as he recognised his mistake in a subsequent paper in 1889.

He leaves the question as to the origin of the bacteroids by budding or otherwise quite undecided, having failed to satisfy himself whether

my suggestion is right or not; at the same time he fully agrees with me and others in believing that these tiny bodies must be the infecting agents, easily and abundantly distributed as they are in the soil, water, &c.

In the 'Botanisches Centralblatt,' vol. 39, No. 12, 1889, p. 356,* is a second paper by Prażmowski, on the nature and biological significance of the root tubercles of the pea, in which he sums up his views so far. He says, the root tubercles of the Papilionaceæ are not normal structures, but are caused by a special fungus, which inhabits the tubercles, and the spores of which must also occur in the soil. Hitherto he had been unable to determine the true nature of this fungus, but only to show that it penetrates through the root-hairs into the young root, grows in it in the form of more or less branched, unseptate tubes, which are clothed by a dense refringent membrane and contain innumerable extremely minute rod-like bodies. Under the influence of this fungus the young tubercle is developed in the deeper parts of the cortex, and in its tissues the bacterium-like contents of the fungus become distributed, and grow, divide, and branch at the expense of the protoplasmic contents. He regarded the phenomenon as one of symbiosis, and as benefiting the host as well as the parasite.

Prażmowski then refers to the papers by Vuillemin, Beyerinck, Hellriegel, and Wilfarth, and says that these instigated him to take up the matter again, and to confine his attention to the pea. The summary of his new results runs as follows:—

The tubercles are not formed in sterilised media unless infected.

The infecting organisms are bacteria, identical in form and properties with those cultivated by Beyerinck from the tubercles of various species. From young tubercles the bacteria can be obtained and cultivated pure, and infections from the cultures cause the tubercles to develop.

The development of the tubercles is only possible in young roots or rootlets; infection does not occur in older portions of the root system.

The tubercle-bacteria penetrate through young (not suberised) cell membranes into the root-hairs and epidermis cells of the root, and there multiply at the expense of the protoplasmic cell-contents. Their further development has so far been observed only in root-hairs. After accumulating and multiplying in the root-hair, they unite in racemose colonies at or near its apex; these colonies become denser and closer, surround themselves with a resistent bright membrane, and join by its means the cell-membrane of the root-hair. Thus arises at the apex of a hair, and on its inner side, a bright knob, usually surrounded by free colonies of bacteria—*i.e.*, colonies not

* This paper is quoted from the 'Berichte a. d. Sitzungen der K. K. Akad. d Wiss. in Krakau,' June, 1889, and it appears in several journals in the same form.

enveloped by membrane. The top of the root-hair coils round the knob, and the latter then puts forth a tube filled with bacteria and surrounded by a brilliant membrane.

From this stage onwards, till the tubercle is developed, this tube behaves like a hypha, growing at the apex, and putting out branches beneath the apex which behave like the original filament.

The tube now grows from the root-hair, through the cortex, and even as far as the endodermis, boring through the walls of the cortex cells, splitting them mostly into two lamellæ, so that a swelling full of bacteria and bounded by lamellæ is formed. In the cells the tube grows towards the nucleus, and usually applies itself so close that the latter is indistinguishable unless stained. Hence, probably, the reason why Beyerinck regarded the tube as "schleim" *débris* remaining over from nuclear division. So far, we have no free bacteria in cells; they are all in the tube.

As soon as the tube reaches the deeper layers of the cortex the cells begin to divide, at first slowly and irregularly, then quickly; and this is especially true of the four or five innermost layers of the cortex. Then also numerous thin branches are developed from the tubes, enter the new cells, and branch in them. The result is the meristem of the tubercle. The middle of the tubercle consists of a parenchyma of larger cells, penetrated by tubes in all planes, and filled later with bacteria freed from the dissolving tubes; the outer layers consist of smaller and more flattened cells with poorer contents, the membranes of which are suberised later. Between the bacteroid tissue and the latter (cortex of tubercle) is a small-celled meristem, free of bacteroids; this produces vascular bundles further back, and these fork and are joined to those of the root. Between the vascular bundles and the bacteroid tissue is further a layer of starch-bearing cells, free of bacteria.

The place where the tubercle forms is predetermined by the infecting tube, and since this enters anywhere, the tubercles arise irregularly, *i.e.*, opposite or not to xylem or phloëm. Pericambium has nothing to do with it, and so Van Tieghem, Beyerinck, and others are wrong in regarding these tubercles as modified lateral roots.

After the tissues of the tubercle are differentiated and the bacteria are set free (parts of the tubes do not burst and dissolve), the latter fuse with protoplasm, multiply by growth and fission, become forked, and subsequently form the bacteroids. The further fate of these depends on their *rôle* in the economy of the plant.

As did Hellriegel, so also Prażmowski put some plants in soils with, others in soils without, nitrogen, and he confirms Hellriegel's results—infected plants require no nitrogen at their roots; non-infected plants pass into a state of hunger and die if not supplied with nitrates.

No decision is arrived at as to whether the nitrogen is got from nitrogen compounds or from the free nitrogen of the air, nor as to what advantage accrues to the bacteria and the host-plant respectively.

As regards the plants' mode of utilising the presence of the bacteria, cultivated bacteria (from pea tubercles) in nutritive media divide indefinitely, and are found there as moving rodlets. In tubercles they are only rodlets while enclosed in tubes; they change their forms in the substance of the protoplasm, becoming forked and developing into bacteroids. As bacteroids they can long go on multiplying by continually developing lateral branches, even in proper nutritive solution ("*In diesem Zustande der Bakteroiden können sie sich noch eine Zeit lang vermehren unter fortwährender Bildung von Seitenzweigen selbst dann, wenn sie aus dem Knöllchen heraus, in geeignete Nährlösung versetzt werden*"). With the further development of the tubercle they become hyaline, cease to multiply, and at length dissolve. The contents of the bacteroid cells are resorbed as the bacteroids dissolve, certain substances being left behind. In other words, the plant utilises the substance of the bacteria.

When emptying begins, and with what energy it proceeds, depend especially on the quantity of nitrogenous compounds at the disposal of the roots. In a soil rich in nitrogen the tubercles go on developing unhindered, become large and typical, and rosy inside, and are not exhausted till late; in poorer soils they attain no great size, are soon emptied, and are green-gray inside.

In both cases the exhaustion proceeds acropetally, from the base onwards. At the apex remains a zone which is not emptied, and its cells are full of bacteria. Moreover, some bacteria in and out of tubes remain in all the cells, and escape during decay into the soil; also animals eat the tubercles and disperse the bacteria. In such injured tubercles the bacteroid masses often envelop themselves anew with membranes, and form smaller and smaller colonies; these the author previously mistook for spores (see p. 438).

From the preceding, we see that the tubercles depend on a symbiosis which is advantageous to both the plant and the bacteria. The latter feed on the sap and cell-contents, and multiply through innumerable generations, and, both during the life of the host and afterwards, become redistributed in the soil. The plant derives advantage in that it obtains nitrogen by means of the bacteria.

Though the symbiosis is useful to both, the plant gains most, for it is the more powerful, and sooner or later overcomes the bacteria, to the multiplication of which it sets limits and finally absorbs the substance of the latter. Being the stronger, the plant directs the symbiosis. It encloses the bacteria in the "*Bakteroidengewebe*," by means of cork, and also protects them. By an apical meristem the

tubercle provides continual successions of new cells for the bacteria to accumulate in while it absorbs the older ones behind. Between the bacteroid tissue and the cork it provides vascular bundles (1) to feed the bacteria and convey the carbohydrates necessary to produce proteids, and (2) to take up the dissolved substance of the bacteroids as required. The thin cell walls of the bacteroid tissue conduce to the same end.

I think it will be admitted by all who study the literature of this subject, that the only real point at issue between Prażmowski and myself is the nature of the bacteroids and their origin from the filaments. I interpreted them as extremely minute budding "gemmules," and not bacteria; Prażmowski, with Beyerinck, regards them as true Schizomycetes. We have all alike failed to actually see the process of budding or fission, a fact which will surprise no one who has examined these extremely minute bodies, which are, as Beyerinck rightly puts it, among the smallest of living beings.

The fact of infection, and the mode of infection, by means of a hypha-like filament passing down the root-hair were definitely established by myself in 1887, and it is satisfactory to find it confirmed in every essential detail by Prażmowski. Our views as to the symbiosis, the struggle between the protoplasm and the "gemmules" (or "bacteroids") are the same: though Prażmowski and Beyerinck carry the matter a step further in definitely inferring the absorption of the conquered bodies of the latter, a point in part supported by some of my experiments.

As to the occurrence, origin, and structure of the tubercles, Prażmowski's account is simply in accordance with my own; and it is interesting to note how many points of detail—the distortions of the root-hairs, the relations of the branching filaments to the nuclei and cell-contents, and those of the incipient tubercle to the end of the filament, for example—are confirmed by him.

There is one point of extreme importance between Beyerinck and Prażmowski on the one hand, however, and myself on the other; they are positive on the subject of the cultivation of the "bacterium" in nutritive media outside the host-plant—or rather the other symbiont—whereas I feel too little confidence in my cultures to assert that the "germ" is definitely isolated and recognised. It is true, I have obtained colonies in the cultures which may be those referred to by these writers, and I may remark that so long ago as 1887 I wrote that certain flocculent clouds in my cultures may be colonies of the organism in question, as I obtain similar clouds of multiplying "germs" on the root-hairs of my water-cultures. Moreover, in some cases I have clear proof that among the colonies in my cultivations the germ in question existed, because I infected peas and beans with them; but it would be going further than the facts

warrant to claim to have definitely isolated and recognised the "germ" by its morphological characters. Still, enough has been done to enable us to say with some confidence that this will yet be done, even if we do not accept that Prażmowski and Beyerinck have already done it.

Presents, November 21, 1889.

Transactions.

Baltimore:—Johns Hopkins University. Studies in Historical and Political Science. Seventh Series. Nos. 7–12. 8vo. *Baltimore* 1889; Annual Report, 1889. 8vo. *Baltimore;* Register for 1888–89. 8vo. *Baltimore* 1889. With Seven Dissertations presented 1888–89. 8vo. *Baltimore.* The University.

Berlin:—K. Preussiche Akademie der Wissenschaften. Abhandlungen. 1888. 4to. *Berlin* 1889. The Academy.

Physikalische Gesellschaft. Verhandlungen. Jahrg. 1888. 8vo. *Berlin* 1889. The Society.

Bern:—Naturforschende Gesellschaft. Mittheilungen. Jahrg. 1888. 8vo. *Bern* 1889. The Society.

Birmingham:—Philosophical Society. Proceedings, Vol. VI. Part 1. 8vo. *Birmingham* [1889]. The Society.

Cambridge, Mass:—American Association for the Advancement of Science. Meeting at Cleveland, 1888. Proceedings. 8vo. *Salem* 1889. The Association.

Canada:—Royal Society of Canada. Proceedings and Transactions, 1888. Vol. 6. 4to. *Montreal* 1889. The Society.

Danzig:—Naturforschende Gesellschaft. Schriften. Bd. VII. Heft 2. 8vo. *Danzig* 1889. The Society.

Davenport:—Academy of Natural Sciences. Proceedings. Vol II. Part 2. Vol. V. Part 1. 8vo. *Davenport* 1880, 1889. The Academy.

Delft:—École Polytechnique. Annales. Tome IV. Livr. 4. Tome V. Livr. 1–2. 4to. *Leide* 1888–89. The School.

Devonshire Association:—Report and Transactions. Vol. XXI. 8vo. *Plymouth* 1889; The Devonshire Domesday. Part 6. 8vo. *Plymouth* 1889. The Association.

Edinburgh:—Royal Physical Society. Proceedings. Session 1887–88. 8vo. *Edinburgh* 1888. The Society.

Liverpool:—Geological Society. Proceedings. Vol. VI. Part 1. 8vo. *Liverpool* 1889. The Society.

London:—Clinical Society. Transactions. Vol. XXII. 8vo. *London* 1889. The Society.

Entomological Society. Transactions, 1889. Part 2–3. 8vo. *London.* The Society.

Transactions (*continued*).

Institution of Mechanical Engineers. Proceedings. 1889. No. 2. 8vo. *London.* The Institution.

Institution of Naval Architects. Transactions. Vol. XXX. 4to. *London* 1889. The Institution.

Iron and Steel Institute. Journal, 1889. 8vo. *London.* The Institute.

Marine Biological Association. Journal. New Ser. Vol. I. No. 2. 8vo. *London* 1889. The Association.

Royal Horticultural Society. Journal. Vol. XI. Part 2. 8vo. *London* 1888–89. The Society.

Royal Statistical Society. Journal. Vol. LII. Part 2–3. 8vo. *London* 1889. The Society.

United Service Institution. Journal. Vol. XXXIII. Nos. 148–150. 8vo. *London* 1889. The Institution.

Manchester:—Geological Society. Transactions. Vol. XX. Part 5–7, 9–10. 8vo. *Manchester* 1889. The Society.

Paris:—École Polytechnique. Journal. Cahier 58. 4to. *Paris* 1889. The School.

Société de Géographie. Bulletin. Sér. 7. Tome X. Trim 1. 8vo. *Paris* 1889. The Society.

Société Entomologique de France. Annales. 1888. 8vo. *Paris* 1888–89. The Society.

Société Française de Physique. Collection de Mémoires relatifs à la Physique. Tome IV. 8vo. *Paris* 1889. The Society.

Philadelphia:—Academy of Natural Sciences. Proceedings. 1889. Part 1. 8vo. *Philadelphia.* The Academy.

Plymouth:—Plymouth Institution. Annual Report and Transactions. Vol. X. Part 2. 8vo. *Plymouth* 1889. The Institution.

San Francisco:—California Academy of Sciences. Memoirs. Vol. II. No. 2. 4to. *San Francisco* 1888; Proceedings. Vol. I. Part 1–2. 8vo. *San Francisco* 1888–89. The Academy.

Sydney:—Linnean Society of New South Wales. Proceedings. Ser. 2. Vol. IV. Part 1. 8vo. *Sydney* 1889; Act of Incorporation, Rules, List of Members, &c. 8vo. *Sydney* 1889. The Society.

Royal Society of New South Wales. Journal and Proceedings. Vol. XXII. Part 2. 8vo. *Sydney* 1888. The Society.

Turin:—Reale Accademia delle Scienze. Memorie. Ser. 2. Tomo XXXIX. 4to. *Torino* 1889. The Academy.

Upsala:—Universitet. Årsskrift. 1888. 8vo. *Upsala.* The University.

Observations and Reports.

Adelaide:—Botanic Garden. Report. 1888. Folio. *Adelaide* 1889. The Director.

Berlin:—Königl. Sternwarte. Berliner Astronomisches Jahrbuch für 1891. 8vo. *Berlin* 1889. The Observatory.

Cambridge, Mass:—Harvard College Observatory. Annals. Vol. XVIII. No. 9. Vol. XIX. Part 1. Vol. XX. Part 2. 4to. *Cambridge* 1889; A Large Photographic Telescope. E. C. Pickering [Sheet, *Cambridge*] 1888; The Bruce Photographic Telescope. E. C. Pickering [Sheet]. *Cambridge* 1889. The Observatory.

Coimbra:—Observatorio Meteorologico e Magnetico da Universidade. Observações Meteorologicas. 1888. Folio. *Coimbra* 1889. The Observatory.

Geneva:—Observatoire. Résumé Météorologique. 1888. 8vo. *Genève* 1889. The Observatory.

India:—Survey of India Department. General Report. 1887–88. Folio. *Calcutta* 1889. The Survey.

Karlsruhe:—Grossherzogliche Sternwarte. Veröffentlichungen. Heft 3. 4to. *Karlsruhe* 1889. The Observatory.

Kiel:—Commission zur Wissenschaftlichen Untersuchung der Deutschen Meere. Sechster Bericht für die Jahre 1887 bis 1889. Jahrg XVII–XIX. Heft 1. Folio. *Berlin* 1889; Atlas Deutscher Meeresalgen. Heft 1. Folio. *Berlin* 1889; Ergebnisse der Beobachtungsstationen. Jahrg. 1888. Heft 1–9. Obl. 4to. *Berlin* 1889. The Commission.

Lisbon:—Observatorio do Infante D. Luiz. Annaes. 1883, 1885–86. Folio. *Lisboa* 1889. The Observatory.

London:—Army Medical Department. Report. 1887. 8vo. *London* 1889. The Department.

Madrid:—Observatorio. Observaciones Meteorológicas. 1884–87. 8vo. *Madrid* 1889. The Observatory.

St. Petersburg:—Comité Géologique. Mémoires. Vol. III. No. 4. Vol. VIII. No. 1. 4to. *St. Petersburg* 1888–89; Bulletin. Tome VII. Nos. 6–10. Tome VIII. Nos. 1–5. Supplément au Tome VIII. 8vo. *St. Petersburg* 1888–89. The Comité.

San Fernando:—Instituto y Observatorio de Marina. Almanaque Náutico. 1891. 8vo. *Madrid* 1889. The Observatory.

Washington:—War Department U.S. Army, Index Catalogue of the Library of the Surgeon-General's Office. Vol. X. 8vo. *Washington* 1889. The Department.

Ball (Sir R. S.), F.R.S. On the Theory of Screws (Excerpt). 4to. *Dublin* 1889. The Author.

Boué (A.) Die Europäische Türkei. 2 vols. 8vo. *Wien* 1889. K. Akademie der Wissenschaften, Wien.

Braithwaite (R.) The British Moss-Flora. Part 12. 8vo. *London* 1889. The Author.

Bristow (H. W.), F.R.S., Obituary Notice, by H. B. Woodward. Extracted from the 'Geological Magazine.' 8vo. *London* 1889. Dr. Woodward, F.R.S.

Brodie (Rev. P. B.) On the Predominance and Importance of the Blattidæ in the Old World. 8vo. *Warwick* [1889]. The Author.

Caruel (T.) Flora Italiana. Vol. VIII. Parte 3. 8vo. *Firenze*, 1889. Prof. Caruel.

Colenso (W.), F.R.S. Ancient Tide-Lore and Tales of the Sea. 8vo. *Napier* 1889. With two Excerpts in 8vo. The Author.

Collins (F. Howard). An Epitome of the Synthetic Philosophy. With a Preface by Herbert Spencer. 8vo. *London* 1889. The Author.

Cooke (J. P.) On a New Method of determining Gas Densities. 8vo. [*Cambridge, Mass.*] 1889. The Author.

Dana (J. D.), For. Mem. R.S. On the Volcanoes and Volcanic Phenomena of the Hawaiian Islands. With a Paper on the Petrography of the Islands, by E. S. Dana. 8vo. [*Newhaven*] 1887–89. Prof. J. D. Dana.

Donders (F. C.), For. Mem. R.S. Het Jubileum van Professor F. C. Donders gevierd te Utrecht. Gedenkboek uitgegeven door de Commissie. 8vo. *Utrecht* 1889. The Committee.

Fleet (J. F.) Corpus Inscriptionum Indicarum. Vol. III. 4to. *Calcutta* 1888. The Government of India.

Foster (M.), Sec. R.S. A Text Book of Physiology. Part 2. 5th ed. 8vo. *London* 1889. The Author.

Gegenbaur (C.) For. Mem. R.S. Morphologisches Jahrbuch. Bd. XIII–XIV; XV, Heft 1–8. 8vo. *Leipzig* 1888–89. Prof. Gegenbaur.

Giglioli (E. H.) Avifauna Italica. 2 vols. 8vo. *Firenze* 1886, 1889; Il Primo Congresso Ornitologico Internazionale tenuto a Vienna 1884: Relazione. 8vo. *Roma* 1885. The Author.

Gordon (H. A.) A Miners' Guide. 8vo. *New Zealand* 1889. The Author.

Gorton (W. C. L.) Line Congruences (Reprint). 8vo. [*Baltimore* 1889]. Johns Hopkins University.

Harcourt (L. F. Vernon.) The Canalization of Rivers and the different Systems of Movable Weirs. A Report (Congrès International de l'Utilisation des Eaux Fluviales). 8vo. *Paris* 1889. With a copy of the same in French. 8vo. The Author.

Hill (S. A.) The Tornadoes and Hailstorms of April and May, 1888, in the Doab and Rohilkhand. 8vo. *Calcutta* 1889. The Author.

Hofmann (A. W. von), F.R.S. Zur Erinnerung an vorangegangene Freunde. 3 vols. 8vo. *Braunschweig* 1888. The Author.

Huygens (Christiaan). Œuvres Complètes. Tome II. 4to. *La Haye* 1889. Société Hollandaise des Sciences.

Johnson (Rev. S. J.) Eclipses and Transits in Future Years. 8vo. *London* 1889. The Author.

Lawes (Sir J. B.), F.R.S. The History of a Field newly laid down to Permanent Grass. 8vo. *London* 1889; Memoranda of the Origin, Plan, and Results of Field and other Experiments at Rothamsted. 4to. [*London*] 1889. The Author.

Lemoine (E.) Sur la Mesure de la Simplicité dans les Tracés Géométriques. 8vo. *Paris* 1889. With two Excerpts in 8vo. The Author.

McCoy (F.), F.R.S. Prodromus of the Zoology of Victoria. Decade 18. 8vo. *Melbourne* 1889. The Government of Victoria.

Macdonald (Rev. D.) Oceania: Linguistic and Anthropological. 8vo. *Melbourne* 1889. The Author.

McIntosh (W. C.), F.R.S. On the Pelagic Fauna of the Bay of St. Andrews during the Months of 1888. 8vo. [1889]. The Author.

Martone (M.) Sulla Risoluzione delle Equazioni Numeriche. 8vo. *Catanzaro* 1889. The Author.

Pflüger (E. F. W.), For. Mem. R.S. Die Allgemeinen Lebenserscheinungen (Rede). 8vo. *Bonn* 1889. The Author.

Price (B.), F.R.S. A Treatise on Analytical Mechanics. Vol. II. 2nd ed. 8vo. *Oxford* 1889. The Author.

Quatrefages (A. de), For. Mem. R.S. Introduction à l'Étude des Races Humaines. 8vo. *Paris* 1889. The Author.

Reade (T. M.) On the Advantage to the Civil Engineer of a Study of Geology. 8vo. [*Liverpool* 1889]. With two Excerpts in 8vo. The Author.

Roscoe (Sir H. E.), F.R.S., and C. Schorlemmer, F.R.S. Ausführliches Lehrbuch der Chemie. Bd. II, IV. Theil 2. 8vo. *Braunschweig* 1889. The Authors.

Schlich (W.) A Manual of Forestry. Vol. I. 8vo. *London* 1889. The India Office.

Sharp (W.), F.R.S. Essays on Medicine. Nos. 55–56. 8vo. *London* 1889. The Author.

Symons (G. J.), F.R.S. British Rainfall. 1888. 8vo. *London* 1889. The Author.

Trimen (R.), F.R.S. (Assisted by J. H. Bowker). South African Butterflies. 3 vols. 8vo. *London* 1887–89. Mr. Trimen.

Wood-Mason (J.) A Catalogue of the Mantodea. No. 1. 8vo. *Calcutta* 1889. The Indian Museum.

November 30, 1889.

ANNIVERSARY MEETING.

Sir G. GABRIEL STOKES, Bart., President, in the Chair.

The Report of the Auditors of the Treasurer's Accounts on the part of the Society was presented, by which it appears that the total receipts on the General Account during the past year, including petty cash balances carried from the preceding year, amounted to £7,433 15*s.* 9*d.*, and that the total receipts on account of Trust Funds, including a balance of £2,112 6*s.* 1*d.* carried from the preceding year, amounted to £4,609 14*s.* 10*d.*; and that the total expenditure in the same period, including an overdrawn balance on the General Account of £953 1*s.* 6*d.* carried from the preceding year, and including also purchase of stock, amounted to £7,328 16*s.* 7*d.* on the General Account, and £2,468 4*s.* 3*d.* on account of Trust Funds, leaving a balance on the General Account of £94 7*s.* 3*d.* at the bankers' and £10 11*s.* 11*d.* in the hands of the Treasurer, and on account of Trust Funds a balance at the bankers' of £2,141 10*s.* 7*d.*

The thanks of the Society were voted to the Treasurer and Auditors.

The Secretary then read the following Lists:—

Fellows deceased since the last Anniversary (Nov. 30, 1888).

On the Home List.

Ball, John, F.L.S.
Bate, Charles Spence, F.L.S.
Bateman, John Frederic La Trobe, M.I.C.E.
Berkeley, Rev. Miles Joseph, F.L.S.
Bristow, Henry William, F.G.S.
Brooke, Sir William O'Shaughnessey Brooke.
De la Rue, Warren, D.C.L.
Halliwell-Phillipps, James Orchard, F.S.A.
Joule, James Prescott, D.C.L.
McDonnell, Robert, M.D.
Newall, Robert Stirling, F.R.A.S.
Parkinson, Rev. Stephen, D.D.
Percy, John, M.D.
Rees, George Owen, M.D.
Robinson, Sir Robert Spencer, Admiral, K.C.B.
Royston-Pigott, George West, M.D.
Tupper, Martin Farquhar, D.C.L.
Williams, Charles James Blasius, M.D.

On the Foreign List.

Chevreul, Michel Eugène.
Donders, Franz Cornelius.

Fellows elected since the last Anniversary.

Aitken, John.	Hughes, Prof. Thomas McKenny, M.A.
Ballard, Edward, M.D.	Poulton, Edward B., M.A.
Basset, Alfred Barnard, M.A.	Sollas, Professor William Johnson, D.Sc.
Brown, Horace T., F.C.S.	Todd, Charles, M.A.
Clark, Latimer, M.I.C.E.	Tomlinson, Herbert, B.A.
Cunningham, Professor David Douglas, M.B.	Worms, Right Hon. Baron Henry de.
Fletcher, Lazarus, M.A.	Yeo, Professor Gerald F., M.D.
Hemsley, William Botting, A.L.S.	
Hudson, Charles Thomas, LL.D.	

The President then addressed the Society as follows:—

In an annual assembling of any body of men as large as that of our Fellows, it must in the course of nature be expected that of those who were or might have been present on one such occasion some will have been removed by death before the next comes round. But the death-roll of the year, which according to our custom is read by the Senior Secretary at our annual meeting, is on this occasion unusually heavy, and the list recalls to us several who have taken an active part in the ordinary work of the Society, and some whose names will be prominently remembered by posterity.

Warren de la Rue, who has repeatedly served on the Council and Committees of the Society, was one of the early pioneers in the application of photography to the delineation and measurement of celestial objects, an application which has now received such great extension. He was one of the party who went to Spain, in 1860, to observe a total solar eclipse, and he took up the department of observation by photography. His results formed the subject of a Bakerian lecture, and are published in the 'Philosophical Transactions' for 1862. They threw much light on the subject of the solar prominences, then, we may say, in its infancy. He devoted much attention to the subject of Sun-spots, and constructed an elaborate machine for their measurement on photographic negatives under the microscope. Several of our Fellows have had the opportunity of seeing the beautiful experiments on electric discharges in rarefied gases which he carried out by means of his magnificent battery of 15,000 chloride of silver cells, the use of which, with his usual urbanity, he accorded to men of science who might be desirous of investigating some point requiring the aid of so costly an appliance.

Charles James Blasius Williams, who died last March, at a very advanced age, was, with one exception, the senior of our Fellows, having been elected in 1835. For many years one of the most prominent physicians in London, after retirement from medical

practice, in the evening of his days, he took up the examination of solar spots, and their possible relation to meteorology.

Stephen Parkinson was well known to Cambridge men as a mathematician, and was the author of several mathematical works in common use in the University.

John Percy was for more than forty years a Fellow of the Society, and has served on the Council. Many years ago I was myself associated with him at the Government School of Mines, where we both were lecturers together. He was a man of accurate knowledge but of a retiring character, and is perhaps best known to the world through his excellent work on metallurgy, the value of which is evidenced by the fact of its having been translated both into French and German.

Owen Rees, also for more than forty years a Fellow of the Society, did good service in the application of chemistry to the elucidation of disease.

Miles Joseph Berkeley, who died at the advanced age of eighty-six, was distinguished as a cryptogamic botanist; indeed in this branch of botanical science he was long looked up to as the leading authority.

Our late Fellow John Ball was for a long time intimately associated with the work of the Society. Besides serving on the Council, he has very frequently assisted us on various Committees. It will be recollected that he was associated with Sir Joseph Hooker in botanical exploration, and the two, at no little personal risk, ascended the Atlas range on the northern side, being the first Europeans who had penetrated so far. Fond of travel, and of mountain climbing, he was led to take up the subject of botany; and in relation to this science, as well as to meteorology and geology, he turned his travels to good account.

George West Royston-Pigott took up the subject of improvements in the microscope, specially as regards the correction of the residue of spherical aberration, and a paper of his on the subject is printed in the 'Philosophical Transactions.'

John Frederic La Trobe-Bateman, who died in June, will long be remembered for his important engineering works, especially in relation to the supply of water to large towns.

William Henry Bristow was associated with De la Beche, Edward Forbes, and other geologists in the early history of the Geological Survey, and remained in that department of the Civil Service of the country, in which he had been promoted to the post of Senior Director, almost up to the end of his life. He particularly distinguished himself by the careful and detailed manner in which he carried out the mapping of the Cretaceous and Jurassic rocks of the South of England. His maps and descriptions of that region have become classical in English geology. Outside of his official work he

published a few papers giving the results of his researches, and was also the author of a useful glossary of mineralogy, as well as of translations of popular geological works.

On the 21st of April, our late Fellow Robert Stirling Newall passed away at the age of eighty-seven. Mr. Newall, as a successful manufacturer, is well known through the improvements which he effected in the construction of iron rope, which rendered him, we may say, one of the chief founders of an important branch of national industry, and through his success in the construction of those submarine cables which play so important a part in the conveyance of intelligence all over the civilized world. But he did not confine himself to the industrial application of scientific principles; he took a leading step in the development of the refracting telescope. At the time of the Exhibition of 1862, the largest refracting telescope in operation was the 16-inch one at Pulkowa. Messrs. Chance, of Birmingham, placed in that exhibition two disks of optical glass, one of flint and one of crown, of far larger size, about 26 inches in diameter. These Mr. Newall, being possessed of ample means, purchased, with the intention of trying what could be done for astronomical observation by the use of a telescope far larger, of its kind, than had hitherto been used. The construction was confided to Cooke, of York, so well known for the excellence of his optical work. The instrument was erected at Mr. Newall's residence at Gateshead, and is pronounced by competent judges to be of first-rate excellence. The atmospheric conditions of Gateshead were not however favourable for the use of so grand an instrument; and shortly before his death Mr. Newall offered it to the University of Cambridge. This generous offer was referred, as is usual in such cases, to a Committee for report. The Committee have issued a provisional report in which they testify to the excellence of the instrument, and recommend its acceptance; but the final arrangements to be proposed are still under consideration.

By the death of James Prescott Joule, the Society has this year lost one of its Fellows whose name will go down to posterity in connexion with his memorable researches on the mechanical equivalent of heat. The circumstances of his birth would naturally have led him to devote himself to commercial pursuits, but the bent of his mind, animated in early years by the instruction he received from the illustrious Dalton, led him to devote himself mainly to the pursuit of science. As in the case of Faraday, his investigations were carried on without the aid of mathematics, at least of the higher mathematics. But, like Faraday, he seemed to have a sort of intuitive apprehension of physical laws. His early scientific studies led him into the domain of electricity, and its connexion with heat; and he showed that when a voltaic current passes through a conducting wire the heat gene-

rated in a given time varies as the resistance multiplied by the square of the current. It was in connexion with magneto-electricity that his first determination was made of the mechanical equivalent of heat, which was confirmed later by its accordance with the equivalent as determined independently altogether of electricity, by measuring on the one hand the work given out by a descending weight, and on the other the heat generated by internal friction in a liquid in which that work was consumed in overcoming resistance. While much may often be done towards discovering the laws of nature by merely qualitative experiments, the final testing of theories which we may have been led to form involves almost always accurate quantitative determinations. Joule as an experimentalist was accurate in quantitative determinations, and his final number for the mechanical equivalent of heat is accepted as a fundamental constant in thermodynamics.

On account of the great importance of Joule's labours, both directly, in the advancement of science, and indirectly, through the knowledge thus acquired, in enabling improvements to be made in the practical application of science for industrial purposes, it has been suggested that it might be desirable to raise some public memorial to him, and the Council has appointed a Committee to consider the question.

Only yesterday our aged Fellow Martin Tupper passed away, who was the author of works which attained a very wide circulation.

I have referred, and that very briefly, to some only of the Fellows whom we have lost during the past year, but fuller details both of them, of other Fellows whom we have lost, and of our recently deceased foreign members will be found in the obituary notices which appear from time to time in the Proceedings, according as they are received from the Fellows who have kindly undertaken to draw them up.

Of those who last year were on our list of Foreign Members, we have since lost one who was truly a veteran in science. More than three years have elapsed since the celebration of the centenary of the birth of M. Chevreul, and two more recurrences of his birthday came round before he was called away. He will be known for his researches on the contrast of colours. But his great work was that by which he cleared up the constitution of the fixed oils and fats, and established the theory of saponification. Few scientific men still surviving were even born when this important research was commenced—a research in the course of which he laid the foundation of the method now universally followed in the study of organic compounds, by showing that an ultimate analysis by itself alone is quite insufficient, and that it is necessary to study the substances obtained by the action of reagents on that primarily presented for investigation.

Our late Foreign Member Franz Cornelius Donders stood in the first

rank among the men of science of our day. He was educated as an army surgeon; but the bent of his mind led him to scientific investigation, and he became one of the most eminent of physiologists as well as the most distinguished ophthalmologist of his day. He contributed powerfully to the advance, not only of ophthalmology, but also of general physiological science; for on whatever physiological subject he touched he left his mark, bringing as he did to bear on it an acute and original mind, thoroughly trained in physical and chemical principles, and a knowledge of the advances made by the foremost among his scientific contemporaries.

There is one whose name, though he was not a Fellow, I cannot pass by in silence on the present occasion. I refer to Thomas Jodrell Phillips Jodrell, who died early in September, in his eighty-second year. About the time of the publication of the reports of the Duke of Devonshire's Commission, the subject of the endowment of research was much talked of, and Mr. Jodrell placed the sum of £6,000 in the hands of the Society for the purpose of making an experiment to see how far the progress of science might be promoted by enabling persons to engage in research who might not otherwise be in a condition to do so. But before any scheme for the purpose was matured, the Government Grant for the promotion of scientific research was started, under the administration of Lord John Russell, then Prime Minister. This rendered it superfluous to carry out Mr. Jodrell's original intention, but he still left the money in the hands of the Society, directing that, subject to any appropriation of the money that he might make, with the approval of the Royal Society, during his lifetime, the capital should immediately upon his death be incorporated with the Donation Fund, and that in the meantime the income thereof should be received by the Royal Society. Of the capital, £1,000 was several years ago assigned to a fund for the reduction of the annual payments to be made by future Fellows, and the remaining £5,000 has now of course been added to the Wollaston Donation Fund. By the Fee Reduction Fund the annual payment of ordinary Fellows elected subsequently to the time of the change was made £3 instead of £4, and the entrance fee abolished. As to the Donation Fund, a very wide discretion was, by the terms of the original foundation, left in the hands of the Council as to the way in which they should employ it in the interest of science.

Since the Croonian Foundation for lectures was put on its present footing, it has been made the means of securing for us the advantage of a lecture delivered before the Society by distinguished foreign men of science. In the present year our Foreign Member M. Pasteur was invited to deliver the lecture. Unfortunately the state of his health would not allow him to deliver it himself, but at one time he hoped

that he would have been able to be present at its delivery. It was ultimately arranged that his fellow labourer at the Pasteur Institute, Dr. Roux, should deliver the Croonian Lecture in his stead; and several of the Fellows have heard his lucid account, first of the discoveries of M. Pasteur in relation to diseases brought about by microscopic organisms, and then further researches of his own in the same field.

In addressing the Fellows at the anniversary last year, I mentioned that Commandant Desforges had kindly offered to compare that portion of Sir George Schuckburgh's scale, with reference to which the length of the seconds pendulum had been determined by Kater and Sabine, with the French standard mètre; and as the ratio of this to the English standard yard was accurately known, the length of the pendulum, as determined by these accurate observers, would thus for the first time be brought into relation with the English yard by direct comparison with accurately compared measures of length. The comparison was shortly afterwards executed, and the scale, which of course was very carefully packed for its journey to Paris and back, has long since been replaced in the apartments of the Society. This highly desirable comparison occupied but a few days in its execution; which affords one example of the scientific advantages derivable, under an international agreement, from the establishment of the Bureau des Poids et Mesures. Our own country, which for some years held aloof from the Convention, forming the sole exception to the general agreement among nations of importance, joined it some years ago; and we thus have the privilege of availing ourselves as occasion may arise of the appliances at the office in Paris for such comparisons of measures of length or weight.

The services of Mr. Arthur Soper, as a special assistant, have been retained during the past session, with advantage to the library. He has completed the much-needed shelf catalogue, and the rearrangement of the books where necessary. In the course of this work the volumes of a purely literary character have been collected together, and a selection of the most valuable have been preserved in a properly protected case. Of the remainder about 150 volumes (in addition to those reported last year) have been presented to various public libraries, and a slip catalogue of the volumes which are retained, containing about 1,700 entries, has been prepared.

The manuscripts (other than the originals of ordinary papers read at the meetings) which have accrued to the Society since the publication of Mr. Halliwell's Catalogue have been collected from various parts of the building into the Archives Room, with the object of preparing a complete catalogue of the manuscripts at present in the possession of the Society.

Since the last anniversary twenty-four memoirs have been published in the 'Philosophical Transactions,' containing a total of 753 pages and 33 plates. Of the 'Proceedings' twelve numbers have been issued, containing 1062 pages and 6 plates. Dr. R. von Lendenfeld's 'Monograph of the Horny Sponges,' mentioned in my last anniversary address, has also been issued during the year in a quarto volume of 940 pages of text and 51 plates.

The Fellows are aware that for a great many years the Royal Society has devoted a part of its funds to the collection, preparation for the press, and correction of the proofs of a Catalogue of Scientific Papers. We have endeavoured to make the work as complete as possible, and to include scientific serials in all languages. The first part, covering the period 1800 to 1863, is printed in six thick quarto volumes, of which the last appeared in 1872. The decade 1864–1873 occupies two more volumes, of which the second was published in 1879. This work, in the preparation of which the Royal Society has spent a large sum, is for the benefit of the whole civilized world, and the sale of it could not be expected nearly to cover the cost of printing, paper, and binding. On a representation to this effect being made to Government, when the first part was ready for the press, the Lords of the Treasury consented that it should be printed at the public expense, the proceeds of the sale of the work, after reserving a certain number of copies for presentation, being repaid to the Treasury. In consideration of the large outlay involved in the preparation, those Fellows of the Society who wished to purchase the work could do so at about two-thirds of the cost to the general public. A similar application to the Treasury with reference to the decade 1864-1873 met with a similar response, and we proceeded, as I mentioned at the anniversary last year, with the preparation of the manuscript for the next decade, 1874–1883, which was then nearly ready. On making application towards the end of last year to the Treasury for the printing of this decade, our request was not acceded to. While declining, however, to continue any further the printing of this great work, the sum of £1,000 was put in the Estimates, and has since been voted by Parliament, to assist us in the publication, and the copies of the work still remaining unsold have been handed over to us. This has enabled us to conclude negotiations with Messrs. Clay and the Syndics of the Cambridge University Press for the printing of the decade last mentioned, and at the same time to make some provision towards the future continuation of the work, without, as it may be hoped, encroaching to a greater extent than hitherto on our own resources.

The utility of the work would obviously be much increased if it could be furnished with some sort of key enabling persons to find what had been written on particular subjects. I am not without

hopes that this very desirable object may yet be accomplished, notwithstanding the magnitude of any such undertaking.

Within the last year the Council of the Royal Society has accepted a duty in connexion with scientific agriculture, of which it will be interesting to the Fellows to be informed. It is well known that for the last fifty years, or thereabouts, Sir John Lawes has carried out on his estate at Rothamsted an elaborate and most persevering series of experiments on the conditions which influence the growth and yield of crops of various kinds, the effect of manures of different kinds, the result of taking the same crop, year after year, from off the same land without supplying to it any manure, &c. Long as these experiments have already been continued, there are questions, particularly as regards the capabilities of the sub-soil, which require for their satisfactory answers that similar experiments should be continued on the same land for a still longer period. In respect of such questions, the investigator of the science of agriculture is in a position resembling that in which the astronomer is often placed, in having to make observations, the full interest of which it must be left to posterity to enjoy.

To prevent the interruption of these experiments, which it would take a life-time to repeat on fresh ground, and at the same time to provide for the carrying out of researches generally bearing on the science of agriculture, Sir John Lawes has created a trust, securing to the trustees a capital sum of £100,000, and leasing to them for ninety-nine years, at a peppercorn rent, certain lands in his demesne on which the experiments have hitherto been carried on, together with his laboratory. The trust is intended to be for original research, not for the instruction of students. The general direction of the experiments and researches to be carried on is vested in a committee of management consisting of nine persons, of whom four are to be appointed by the President and Council of the Royal Society.

The trustees named in the deed were Sir John Lubbock, Dr. Wells, and our treasurer, Dr. Evans. One of these is now no more. Lord Walsingham has been appointed a trustee in place of the late Dr. Wells.

The Copley Medal for the year has been awarded to Dr. Salmon for his various papers on subjects of pure mathematics, and for the valuable mathematical treatises of which he is the author. Dr. Salmon's published papers are all valuable. Among others may be mentioned his researches on the classification of curves of double curvature, and on the condition for equal roots of an equation; the very important theorem of the constant anharmonic ratio of the four tangents of a cubic curve; his researches on the theory of reciprocal surfaces; his paper on quaternary cubics. But any notice of his contributions to the advancement of pure mathematics would be incom-

plete which did not specially mention his invaluable text-books on conic sections, higher plane curves, solid geometry, and the modern algebra—works which not only give a comprehensive view of the subjects to which they relate, but contain a great deal of original matter.

Of the Royal Medals, it is the usual though not invariable practice to award one for mathematics or physics, including chemistry, and one for some one or more of the biological sciences. No distinction is, however, made between the two medals in point of order of precedence, and I will, therefore, take the names of the medallists in alphabetical order.

The Council have awarded one of the Royal Medals this year to Dr. Walter Holbrook Gaskell for his researches in cardiac physiology, and his important discoveries in the anatomy and physiology of the sympathetic nervous system.

In his memoir, "On the Rhythm of the Heart of the Frog" (Croonian Lecture, 'Phil. Trans.,' 1882), and in a subsequent memoir, "On the Innervation of the Heart of the Tortoise" ('Journ. of Physiol.,' vol. 4), Dr. Gaskell very largely advanced our knowledge of the physiology of the heart-beat, more especially as relates to the sequence of the beats of the several parts, the nature of the inhibitory action of the vagus nerve, and the relations of tonicity and conducting power to rhythmical contraction. These memoirs, however, lacked completeness on account of their not taking into full consideration the action of the cardiac augmentor or accelerator fibres, the existence of which had been previously indicated in the case of mammals, and suspected in the case of the frog and allied animals.

By a striking experiment ('Journ. of Physiol.,' vol. 5) Dr. Gaskell subsequently gave the first clear demonstration of the presence in the frog of cardiac augmentor fibres; also he gave a clear account of the nature of the action of these fibres, and the relations of that action to the action of the vagus fibres. Revising his previous work by the help of the light thus gained, Dr. Gaskell was enabled to give the first really consistent and satisfactory account of the nature of the heart-beat, of the modifications of beat due to extrinsic nerves, and of the parts played by muscular and nervous elements respectively.

Important as was this work on the heart, Dr. Gaskell's subsequent work "On the Structure, Functions, and Distribution of the Nerves which govern the Vascular and Visceral Systems" ('Journ. of Physiol.,' vol. 7) has a far higher importance and significance. In spite of the knowledge which during the past thirty or forty years has been gained concerning vaso-motor nerves and the nerves governing the movements of the viscera, physiologists had up to the time of the appearance of Dr. Gaskell's memoir failed to obtain a clear conception

of the nature and relations of the so-called sympathetic nervous system. By his researches, in which the several methods of gross anatomical investigation, minute histological examination, and experimental inquiry were, in a striking manner, made to assist each other, Dr. Gaskell, by tracing out the course and determining the nature of vaso-constrictor and vaso-dilator fibres, and comparing them with the cardiac augmentor and inhibitory fibres, and with the fibres governing the visceral muscles, has already reduced to order what previously was to a large extent confusion, and has opened up what promises to be the way to a complete understanding of the whole subject.

The results arrived at, besides their great physiological importance, on the one hand promise to be of great assistance in practical medicine, and on the other are eminently suggestive from a purely morphological point of view.

The other Royal Medal has been awarded to Professor Thomas Edward Thorpe for his researches on fluorine compounds, and his determination of the atomic weights of titanium and gold.

Professor Thomas Edward Thorpe's experimental work has secured for him a place in the first rank of living experimentalists.

His researches, which are not confined to one department of chemical science, but extend over many branches, are all distinguished, both by accuracy and originality of treatment. As examples of the high character of his investigations those of the determinations of the atomic weights of titanium and gold may be specially cited as permanently settling the value of two most important chemical constants; whilst his researches on the fluorine compounds, including the discovery of thiophosphoryl fluoride, a body capable of existing undecomposed in the state of gas, and his latest work on the Vapour-density of Hydrofluoric Acid, do not fall short of the highest examples of classical chemical investigation.

The Davy Medal has been awarded to Dr. W. H. Perkin for his researches on magnetic rotation in relation to chemical constitution.

Dr. Perkin is well known as the originator of what is now a great industry, that of the coal-tar colours, by his preparation and application to tinctorial purposes of a colouring matter which had previously merely been noticed as affording a chemical test for the presence of aniline. This, however, is now a long time ago, and it is for more recent work, the interest of which is purely scientific, that the medal has been awarded to him.

Dr. Perkin first showed, in 1884, that a definite relationship exists between the chemical constitution of substances and their power of rotating the plane of polarisation of light when under magnetic influence; and he pointed out how the "molecular coefficient of magnetic rotation" or "molecular rotatory power" might be deduced.

In 1884 he presented to the Chemical Society a lengthy paper describing his method and the results obtained for a very large number of paraffinoid hydrocarbons and haloid and oxygenated derivatives thereof; from these he deduced "constants," which he has since shown to be applicable in calculating the magnetic rotatory power of paraffinoid compounds generally. From time to time he has published further instalments of his work, and only quite recently has described the results obtained on examining nitrogen compounds, which exhibit many most interesting peculiarities.

The results are of special value on account of the exceptional care devoted to the preparation of pure substances, and the guarantee which Dr. Perkin's reputation affords, that everything possible has been done to secure accuracy; and also because the substances chosen are for the most part typical substances, or belong to series in which a simple relationship exists.

The Statutes relating to the election of Council and Officers were then read, and Sir W. Aitken and Professor H. G. Seeley having been, with the consent of the Society, nominated Scrutators, the votes of the Fellows present were taken, and the following were declared duly elected as Council and Officers for the ensuing year:—

President.—Sir George Gabriel Stokes, Bart., M.A., D.C.L., LL.D.

Treasurer.—John Evans, D.C.L., LL.D.

Secretaries.— { Professor Michael Foster, M.A., M.D.
The Lord Rayleigh, M.A., D.C.L.

Foreign Secretary.—Archibald Geikie, LL.D.

Other Members of the Council.

Professor Henry Edward Armstrong, Ph.D.; Professor William Edward Ayrton; Charles Baron Clarke, M.A.; Professor W. Boyd Dawkins, M.A.; Edward Emanuel Klein, M.D.; Professor E. Ray Lankester, M.A.; Hugo Müller, Ph.D.; Professor Alfred Newton, M.A.; Captain Andrew Noble, C.B.; Rev. Stephen Joseph Perry, D.Sc.; Sir Henry E. Roscoe, D.C.L.; Edward John Routh, D.Sc.; William Scovell Savory; Professor Joseph John Thomson, M.A.; Professor Alexander William Williamson, LL.D.; Sir Charles William Wilson, Col. R.E.

The thanks of the Society were given to the Scrutators.

Balance Sheet. 1889.

***Statement of Receipts and Expenditure from November* 12th, 1888, *to November* 12th, 1889.**

	£	s.	d.	£	s.	d.
To Balance in hand, Catalogue Account	14	1	7			
,, ,, Petty Cash	8	1	4	22	2	11
,, Annual Contributions, 158 at £4	632	0	0			
,, 140 at £3	420	0	0	1,052	0	0
,, Admission Fees				10	0	0
,, Fee Reduction Fund, in lieu of Admission Fees and Annual Contributions				290	0	0
,, Rents:						
Fee Farm, Lewes	18	14	5			
Mablethorpe Estate	88	4	0	106	18	5
,, Ground Rents				609	12	5
,, Dividends (exclusive of Trust Funds)				1,916	19	4
,, do. Jodrell Fund				133	13	11
,, Interest on Mortgage Loan				585	0	0
,, Sale of Transactions and Proceedings				677	11	6
,, Treasury Grant on account of Catalogue				1,000	0	0
,, Sale of Catalogue				188	6	6
,, Sale of Krakatoa Report (leaving £100 12*s.* 3*d.* Expenditure in excess of Receipts)				416	4	10
,, Transfer from Handley Fund on account of Catalogue				87	2	4
,, Compositions				120	0	0
,, Eclipse Expedition—Government Grant of Balance				207	11	7
,, Limsil				4	12	0
,, Sale of Lendenfeld Monograph				6	0	0
				£7,433	15	9

	£	s.	d.	£	s.	d.
By Bankers, Balance overdrawn, 12th November, 1888				953	1	6
,, Salaries, Wages, and Pension				1,726	8	4
,, Catalogue of Scientific Papers				166	14	0
,, Books for the Library				319	18	11
,, Printing and Advertising Transactions, and Separate Copies to Authors and Publisher	476	6	7			
,, Ditto Proceedings, Nos. 271 to 279	432	6	4			
,, Ditto Miscellaneous	137	17	6			
,, Paper for Transactions and Proceedings	248	12	4			
,, Binding ditto	43	2	0			
,, Engraving and Lithography	687	0	10	2,025	5	7
,, Soirée and Reception Expenses				137	7	6
,, Coal, Lighting, &c.	231	12	2			
,, Office Expenses	48	12	7			
,, House Expenses	119	14	7			
,, Tea Expenses	15	12	2			
,, Fire Insurance	56	15	0			
,, Taxes	44	12	6			
,, Advertising	20	0	6			
,, Postage, Parcels, and Petty Charges	63	3	0			
,, Miscellaneous Expenses	42	16	8	642	19	2
,, Law Charges				5	0	8
,, Lendenfeld Monograph (making with previous Expenditure £882 2*s.* 7*d.*)				585	13	5
,, Atlas Company Leasehold Assurance on Account of Catalogue				744	2	6
,, Carrington Donation				22	10	0
,, Balance at Bankers				94	7	3
,, Balance on hand, Catalogue Account	£2	17	7			
,, Ditto, Petty Cash	7	14	4	10	11	11
				£7,433	15	9

Trust Funds.

	£	s.	d.	£	s.	d.
To Balance at Bank 12th November, 1888:—						
General Account	1,463	14	1			
Fee Reduction Fund Account	296	11	2	2,112	6	1
Scientific Relief Fund Account	352	0	10			
,, Scientific Relief Fund, Dividends, and Donations	646	12	6			
,, Donation Fund, Dividends, and Transfer from Jodrell Fund	262	3	9			
,, Rumford Fund, Dividends	65	2	2			
,, Bakerian and Copley Medal Fund, Dividends, &c.	56	10	8			
,, Keck Bequest, Dividends	23	8	0			
,, Wintringham Fund, Dividends, &c.	33	13	0	2,497	8	9
,, Croonian Lecture Fund—Rent	50	9	8			
,, Davy Medal Fund, Dividends	32	3	6			
,, Gassiot Trust, Dividends, &c.	498	7	2			
,, Handley Fund, Dividends	187	2	4			
,, Jodrell Fund, Dividends	145	5	6			
,, Fee Reduction Fund, Dividends	414	12	6			
,, Darwin Memorial Fund, Dividends	81	18	0			
				£4,609	14	10

	£	s.	d.	£	s.	d.
By Scientific Relief Fund, Grants	228	0	0			
,, Donation Fund, Grants	243	18	0			
,, Rumford Fund Medal and Gift	136	4	9			
,, Bakerian and Copley Medal Fund, Medal, and Gift	58	12	0			
,, Keck Bequest, Payment to Foreign Secretary	23	8	0			
,, Wintringham Fund, Payment to Foundling Hospital	37	19	9			
,, Croonian Lecture Fund, Payments	44	13	8			
,, Davy Medal Fund, Gold Medals	32	4	6	2,468	4	3
,, Gassiot Trust, Payments to Kew Committee and Investment	535	18	9			
,, Handley Fund, Payments	187	2	4			
,, Jodrell Fund, Transfer to Royal Society General Account and to Donation Fund	145	5	6			
,, Fee Reduction Fund, Investment and transfer to Royal Society General Account (1889)	512	5	0			
,, Darwin Memorial Fund, Expenses and Investment	282	12	0			
,, Balance at Bankers:—						
General Account	1,171	18	7			
Fee Reduction Fund Account	198	18	8	2,141	10	7
Scientific Relief Fund Account	770	13	4			
				£4,609	14	10

Estates and Property of the Royal Society, including Trust Funds.

Estate at Mablethorpe, Lincolnshire (55A. 2R. 2P.), rent £100 per annum.

Ground Rent of House No. 57, Basinghall Street, rent £380 per annum.

" " of 23 houses in Wharton Road, West Kensington, rents £253 per annum.

Fee Farm Rent, near Lewes, Sussex, £19 4*s.* per annum.

One-fifth of the clear rent of an estate at Lambeth Hill, from the College of Physicians, about £52 per annum, Croonian Lecture Fund.

Stevenson Bequest. Chancery Dividend. One-fourth annual interest on Government Annuities and Bank Stock £15,000 (produced £544 5*s.* 8*d.* in 1888–89).

£15,000 Mortgage Loan, 4 per Cent.

£14,224 8*s.* 3*d.*, 2¼ per Cent. Consolidated Stock, being £10,772 7*s.* 2*d.* on account of the following Funds:—

	£	*s.*	*d.*
Rumford Fund	2,322	19	0
Wintringham Fund	1,200	0	0
Gassiot Trust	400	0	0
Sir J. Copley Fund	1,666	13	4
Jodrell Fund	5,182	14	10

and £3,452 1*s.* 1*d.* in Chancery, arising from sale of the Coleman Street Estate.—General Purposes.

£403 9*s.* 8*d.* New 2½ per Cent. Stock.—Bakerian and Copley Medal Fund.

£1,000 India 3½ per Cent. Stock.—General Purposes.

£600 Midland Railway 4 per Cent. Debenture Stock.—Keck Bequest.

£5,660 Madras Railway Guaranteed 5 per Cent. Stock { General Purposes, £5,000. Davy Medal Fund, £660.

£10,000 Italian Irrigation Bonds.—The Gassiot Trust.

£6,396 Great Northern Railway 4 per Cent. Debenture Stock { Scientific Relief Fund, £5,000. The Trevelyan Bequest, £1,396.

£4,200 Metropolitan 3½ per Cent. Stock.—Fee Reduction Fund.

£7,000 London and North Western Railway 4 per Cent. Perpetual Debenture Stock.—Fee Reduction Fund.

£18,150 " " " " 4 per Cent. Consolidated Guaranteed Stock.— { £6,000 Scientific Relief Fund. £12,150 General Purposes.

£5,000 North Eastern Railway 4 per Cent. Preference Stock.—General Purposes.

£5,000 London and North Western Railway Consolidated 4 per Cent. Preference Stock.—General Purposes.

£2,200 South Eastern Railway 4 per Cent. Debenture Stock.—Darwin Memorial Fund.

£4,340 South Eastern Railway 5 per Cent. Debenture Stock.—Scientific Relief Fund.

£3,333 London and South Western Railway 4 per Cent. Preference Stock.—General Purposes.

£5,030 Great Northern Railway Perpetual 4 per Cent. Guaranteed Stock.—Donation Fund.

£4,798 Lancashire and Yorkshire Railway 4 per Cent. Guaranteed Stock.—Handley Fund.

£1,000 Policy in the Atlas Assurance Office, becoming due October 7th, 1899.—Catalogue Account.

JOHN EVANS, *Treasurer.*

We, the Auditors of the Treasurer's Accounts on the part of the Council, have examined these Accounts and found them correct.

WILLIAM POLE.
ARTHUR W. RÜCKER.

We, the Auditors of the Treasurer's Accounts on the part of the Society, have examined these Accounts and found them correct.

A. R. CLARKE.
JAMES COCKLE.
JOHN RAE.

Trust Funds. 1889.

Scientific Relief Fund.

£6,000 L. & N.W.R. 4 per Cent. Consolidated Guaranteed Stock.
£5,000 Great Northern Railway 4 per Cent. Debenture Stock.
£4,340 South Eastern Railway 5 per Cent. Debenture Stock.

Dr.	£	s.	d.	£	s.	d.		*Cr.*				£	s.	d.
To Balance { Income	388	13	1				By Grants					228	0	0
Less — Capital over-invested	36	12	3	352	0	10	" Balance in hand, Income	£801	4	7	}	770	13	4
" Dividends				640	11	6	" Less—Capital over-invested	30	11	3	}			
" Annual Subscriptions				6	1	0								
				£998	13	4						£998	13	4

Donation Fund.

£5,030 Great Northern Railway Perpetual 4 per Cent. Guaranteed Stock.
The Trevelyan Bequest. £1,396 Great Northern Railway 4 per Cent. Debenture Stock.

	£	s.	d.		£	s.	d.
To Balance	532	5	2	By Grants	243	18	0
" Dividends	250	12	2	" Balance	550	10	11
" Transfer from Jodrell Fund	11	11	7				
	£794	8	11		£794	8	11

Rumford Fund.

£2,322 19*s*. 2¾ per Cent. Consolidated Stock.

	£	s.	d.		£	s.	d.
To Balance	158	10	9	By Medals	59	4	9
„ Dividends	65	2	2	„ Payment to P. Tacchini	77	0	0
				„ Balance	87	8	2
	£223	12	11		£223	12	11

Bakerian and Copley Medal Fund.

Sir Joseph Copley's Gift, £1,666 13*s*. 4*d*. 2¾ per Cent. Consolidated Stock.
£403 9*s*. 8*d*. New 2½ per Cent. Stock.

	£	s.	d.		£	s.	d.
To Balance	121	5	10	By Gold Medal	4	12	0
„ Dividends, New 2½ per Cent. Stock	9	16	8	„ Professor T. H. Huxley—Sir J. Copley's Gift	50	0	0
„ Dividend—Sir J. Copley's Fund	46	14	0	„ Professors Rücker and Thorpe—Bakerian Lecture	4	0	0
				„ Balance	119	4	6
	£177	16	6		£177	16	6

The Keck Bequest.

£600 Midland Railway 4 per Cent. Debenture Stock.

	£	s.	d.		£	s.	d.
To Dividends	23	8	0	By Payment to Foreign Secretary	23	8	0

Wintringham Fund.

£1,200 2½ per Cent. Consolidated Stock.

	£	s.	d.		£	s.	d.
To Balance	46	15	3	By Payment to Foundling Hospital	37	19	9
„ Dividends	33	13	0	„ Balance	42	8	6
	£80	8	3		£80	8	3

Croonian Lecture Fund.

One-fifth of the clear rent of an Estate at Lambeth Hill, from the College of Physicians, about £52 per annum.

	£	s.	d.		£	s.	d.
To Rent	50	9	8	By Balance at November, 1888	5	16	0
				„ Lecture—Professor Kühne	44	13	8
	£50	9	8		£50	9	8

Davy Medal Fund.

£660 Madras Railway Guaranteed 5 per Cent. Stock.

	£	s.	d.		£	s.	d.
To Balance	77	1	1	By Gold Medals	32	4	6
„ Dividends	32	3	6	„ Balance	77	0	1
	£109	4	7		£109	4	7

The Gassiot Trust.

£10,000 Italian Irrigation Bonds.

£400 2¾ per Cent. Consolidated Stock.

	£	s.	d.		£	s.	d.
To Balance	52	0	6	By Payments to Kew Committee	487	10	0
„ Dividends	498	7	2	„ Purchase of £50 2¾ per Cent. Consolidated Stock	48	8	9
				„ Balance	14	8	11
	£550	7	8		£550	7	8

Handley Fund.

£4,798 Lancashire and Yorkshire Railway 4 per Cent. Guaranteed Stock.

	£	s.	d.		£	s.	d.
To Dividends	187	2	4	By Institut Pasteur	100	0	0
				„ Transfer to Catalogue Account	87	2	4
	£187	2	4		£187	2	4

The Jodrell Fund.

£5,182 14*s.* 10*d.* 2¾ per Cent. Consolidated Stock.

	£	s.	d.		£	s.	d.
To Dividends	145	5	6	By Transfer to Royal Society General Account	138	13	11
				„ „ „ Donation Fund—Proportion of Dividends since 3rd September	11	11	7
	£145	5	6		£145	5	6

Fee Reduction Fund.

£4,200 Metropolitan 3½ per Cent. Stock.
£7,000 London and North Western Railway 4 per Cent. Perpetual Debenture Stock.

	£	s.	d.		£	s.	d.
To Balance	296	11	2	By Transfer to Royal Society General Account	290	0	0
" Dividends	414	12	6	" Purchase of £200 Metropolitan 3½ per Cent. Stock	222	5	0
				" Balance	198	18	8
	£711	8	8		£711	8	8

Darwin Memorial Fund.

£2,200 South Eastern Railway 4 per Cent. Debenture Stock.

	£	s.	d.		£	s.	d.
To Balance { Capital, £249 1 8; Income, £232 9 10 }	481	11	6	By Printing and Engraving	22	15	6
" Dividends	81	18	0	" Purchase of £200 S.E.R. 4 per Cent. Debenture Stock	259	16	6
				" Balance, Income £291 12 4; Less Capital over-invested.... £10 14 10	280	17	6
	£563	9	6		£563	9	6

The following Table shows the progress and present state of the Society with respect to the number of Fellows :—

	Patron and Royal.	Foreign.	Compounders.	£4 yearly.	£3 yearly.	Total.
Nov. 30, 1888 ..	5	49	182	160	127	523
Since Elected ..	..	..	+ 2	+ 1	+ 13	+ 16
Since Deceased ..	..	− 2	− 8	− 10	− 1	− 21
Nov. 30, 1889 ..	5	47	176	151	139	518

Account of the appropriation of the sum of £4,000 (the Government Grant) annually voted by Parliament to the Royal Society, to be employed in aiding the advancement of Science (continued from Vol. XLV, p. 69).

1888–89.

	£
Dr. R. Stockman and D. B. Dott, for a Research into the Chemical Properties and Physiological Action of Bodies derived from some Alkaloids by Substitution or Decomposition	25
J. V. Jones, for the Measurement of an Electrical Resistance in Absolute Measure by the method of Lorenz	50
A. P. Laurie, for a Research on the Properties of Alloys tested in Voltaic Cells, and replacing the Zinc Plate therein..	20
F. R. Japp, for an Investigation of the Reactions of Ketones, Diketones, and Allied Compounds	75
A. M. Worthington, for an Investigation into the Tensile Strength of Liquids at different Temperatures, and into the Relation between Stress and Strain in a Stretched Liquid....	20
Hon. R. Abercromby, for the Investigation of British Thunderstorms	25
A. R. Ling, for the Study of the Halogen Derivatives of Quinones	30
G. T. Moody, for the Investigation of Isomeric Xylene Derivatives..	25
G. J. Symons, for completing the Collection of British Rainfall Records for the 17th and 18th Centuries	50
Carried forward................	£320

	£	s.	d.
Brought forward	320	0	0
C. I. Burton, for a Research on the Heat produced by Compressing Solids and Liquids	50	0	0
J. A. Ewing, for Researches on the Magnetic Qualities of Iron and other Magnetic Metals....................	50	0	0
T. Carnelley and A. Thompson, for an Investigation of the Relation of Solubility to Fusibility, more particularly in the case of Isomeric Organic Compounds	25	0	0
H. R. Mill, for completion of the Discussion of Observations on the Temperature of the Water in the Clyde Sea Area and the Sea-lochs of the West of Scotland	25	0	0
S. Skinner, for continuation of Researches upon the Substances produced by the Action of Phenylhydrazine on Urea Derivatives	30	0	0
J. T. Bottomley, for continuation of Research on Radiation of Heat	100	0	0
A Committee of the Royal Society. Balance of Expenses of the Solar Eclipse (1886) Expedition........	207	11	7
G. S. Brady and A. M. Norman, for expense of Plates to a Memoir on the European and North Atlantic Ostracoda..	50	0	0
L. C. Wooldridge, for further Research on a New Mode of Protection against Zymotic Disease	100	0	0
Liverpool Marine Biological Committee, for the continued Scientific Exploration of the L.M.B.C. District, Liverpool Bay, and to aid Mr. I. C. Thompson in his Research on the Surface Fauna, and especially the Copepoda	50	0	0
T. Johnson, for the Investigation of a Number of Obscure or Unknown Points in the Florideæ............	30	0	0
H. G. Seeley, for an Investigation of the Permian or Trias Reptilia in Russia in Europe, and Cape Colony	200	0	0
G. Massee, to complete a Monograph of the Fungi belonging to the order Thelephorei......................	50	0	0
O. J. Lodge, for Apparatus to be used in an Investigation into the Discharge of Condensers, and the Phenomena attending Sparks	25	0	0
J. Joly, for a Research on the Specific Heats of Gases at Constant Volume	60	0	0
S. P. Thompson, for the construction of an Instrument to determine with precision the Focal Planes and Principal Planes of Optical Combinations of Lenses having a Short Focus	35	0	0
Carried forward..............	£1,407	11	7

	£	s.	d.
Brought forward..........	1,407	11	7
W. J. Dibdin, for a Research on Stellar Photometry, especially the Determination of the Luminous Energy of the Coloured Stars..................................	100	0	0
Royal Astronomical Society, for sending Observers to Cayenne and Autigas Factories (South Africa), to make Observations, especially Photographic, of the Total Solar Eclipse of December 21–22, 1889	400	0	0
A. A. Common, for payment of an Assistant in applying his 5-foot Telescope to Astronomical Photography....	150	0	0
J. A. Fleming, to test a proposed method of constructing a Standard Air Condenser, to be available for the Comparison of the Mica and Paraffin Condensers used in Telegraph work..................................	50	0	0
J. N. Lockyer, for payment of an Assistant to carry on Spectroscopic Observations	125	0	0
N. Collie, for a Research on (1) the Action of HCl on the Amide of Acetoacetic Ether, &c.; (2) the Constitution of some Oxypyridine Derivatives..............	25	0	0
S. E. Linder and H. Picton, for a Research into the Formation and Properties of Metallic Hydrosulphides, &c..	30	0	0
J. E. Reynolds, for continuation of Researches on Silicon Compounds..	60	0	0
W. R. Dunstan, for a Research on the Connection between Chemical Constitution and Physiological Action as exhibited by some of the Homologous and Metameric Nitrites of the Paraffin Series.........................	75	0	0
W. R. Dunstan, for further Aid in an Examination of the Reduction Products of the Nitro-paraffins	20	0	0
Dr. C. R. Alder Wright, for Aid in continuing Researches on Ternary Alloys...	100	0	0
G. J. Symons, for a Research on the Temperature of Hot Springs in the Pyrenees	50	0	0
C. Lapworth, for continuing a Research among the Lower Palæozoic Rocks and Fossils of Britain	100	0	0
West Indies Committee, for further Aid in collecting Fauna and Flora in the less known West India Islands ..	100	0	0
China Floral Committee, for continuing the Investigation of the Flora of China..........................	300	0	0
G. H. Fowler, for a Research on Problems connected with the Physiology and Binomics of the Lesser Crustacea	150	0	0
Carried forward	£3,242	11	7

	£	s.	d.
Brought forward	3,242	11	7
E. R. Lankester, for obtaining Embryos of *Amphioxus*, and for the payment of an Assistant in studying the same	100	0	0
W. D. Halliburton, for continuation of Researches in Physiological Chemistry	50	0	0
V. Horsley (with Messrs. Beevor, Spencer, Dean, and Gotch), for Researches into the Functions of the Central Nervous System...................................	150	0	0
J. R. Bradford, for a Research into the Nature of Secretion, more especially the Nature of Secreting Nerves....	50	0	0
E. A. Schäfer, for a Research into the Functions of certain parts of the Central Nervous System............	100	0	0
Dr. T. L. Brunton, for Materials and Assistants for a Research on the Connection between Chemical Constitution and Physiological Action	200	0	0
J. A. McWilliam, for a further Investigation into the Innervation of the Mammalian Heart	30	0	0
J. Gnezda, a loan of Instruments.			
A. Lingard, for continuing Investigations into the Protection against Infectious Diseases, more especially Anthrax and Tubercle, from the point of view of the relation existing between the Fœtus and its Mother	40	0	0
W. F. Denning, for the Observation, and discussion of Observations, of Shooting Stars, with particular reference to Stationary Radiant Points	20	0	0
S. U. Pickering, for a further Investigation on the Nature of Solutions	50	0	0
P. Frankland, to continue Investigations on the Chemical Changes effected by Specific Micro-organisms..	50	0	0
C. A. Ballance and S. Shattock, for further Research on the Pathology of Cancer	50	0	0
Piazzi Smyth, to obtain Ultra Definition and Extreme Separation in Luminous Spectroscopy	50	0	0
C. S. Sherrington, to assist in his Researches on the Nervous System....................................	50	0	0
Scottish Meteorological Society, for Aid in a systematic Observation at Ben Nevis Observatory of the number of Dust Particles in the Atmosphere	50	0	0
	£4,282	11	7

Dr.	£	s.	d.	Cr.	£	s.	d.
To Balance, November 30, 1888	209	19	11	By Appropriations, as above	4,282	11	7
„ Grant from Treasury	4,000	0	0	Salaries, Printing, Postage, Advertising, and other Administrative Expenses	113	12	1
„ Repayments	210	0	0	By Balance, Nov. 30, 1889	61	2	4
„ Interest on Deposit	37	6	1				
	£4,457	6	0		£4,457	6	0

Account of Grants from the Donation Fund in 1888–89.

	£	s.	d.
Prof. T. R. Jones, for illustrations of his work on the Fossil Astracoda, £25. On account	18	18	0
Prof. W. N. Parker, to assist in Researches on *Protopterus annectens*	25	0	0
Prof. Schäfer, to assist in Researches on the Functions of certain parts of the Brain	25	0	0
Dr. J. Rose Bradford, to assist in his Researches on the Vaso-motor Nerves of the Lungs and the Kidneys	25	0	0
Prof. D'Arcy Thompson, to assist in purchasing specimens of Natural History through Dundee Whalers	35	0	0
Dr. Geikie, to assist the Rev. R. Baron in his Geological Researches in Madagascar	20	0	0
Sir J. D. Hooker, for payment of an Artist to assist in illustrating a Monograph of the Indian Orchids	50	0	0
Prof. W. K. Parker, to assist in Researches on the Morphology of the Vertebrata	25	0	0
W. de la Rue, for the completing of his Catalogue of Latitude and Longitude of Solar Spots, £200. On account	20	0	0
	£243	18	0

Report of the Kew Committee for the Year ending October 31, 1889.

The operations of The Kew Observatory, in the Old Deer Park, Richmond, Surrey, are controlled by the Kew Committee, which is constituted as follows:

Mr. F. Galton, *Chairman.*

Captain W. de W. Abney, C.B., R.E.	The Earl of Rosse.
Prof. W. G. Adams.	Prof. A. W. Rücker.
Staff-Commander E. W. Creak, R.N.	Mr. R. H. Scott.
Prof. G. C. Foster.	Lieutenant-General R. Strachey, C.S.I.
Admiral Sir G. H. Richards, K.C.B.	General J. T. Walker, C.B.
	Captain W. J. L. Wharton, R.N.

The work at the Observatory may be considered under the following heads:—

1st. Magnetic observations.
2nd. Meteorological observations.
3rd. Solar observations.
4th. Experimental, in connexion with any of the above departments.
5th. Verification of instruments.
6th. Rating of Watches and Marine Chronometers.
7th. Miscellaneous.

The Royal Society's Committee of the Kew Observatory, since the issue of the last Report, have lost by the death of Mr. de la Rue a colleague who for nearly forty years had taken a prominent part in the management of the Observatory, and who had long presided over the Committee as their Chairman. Mr. de la Rue was one of the most munificent benefactors of the Observatory. It was at his suggestion that the first photoheliograph was constructed and brought into use under his supervision at the Observatory. His sound practical judgment and thorough familiarity with scientific operations of all kinds were of constant service to the Committee, and his loss will be greatly felt by them.

I. Magnetic Observations.

No change has been made in the magnetographs during the past year. The curves representing Declination, Horizontal Force, and Vertical Force variations have been obtained uninterruptedly, and, as in former years, the scale values of all the instruments were determined in the month of January.

The ordinates of the various photographic curves were then ascertained to be as follows:—

Declination: 1 inch $=0^\circ\ 22'\cdot04$. 1 cm. $=0^\circ\ 8'\cdot7$.

Bifilar, January 15, 1889,	for 1 inch δH	$=$	0·0278 foot grain unit.
	„ 1 cm. „	$=$	0·00050 C.G.S. unit.
Balance, January 16, 1889	„ 1 inch δV	$=$	0·0285 foot grain unit.
	„ 1 cm. „	$=$	0·00052 C.G.S. unit.

The principal magnetic disturbance of the year was recorded on the morning of July 16th; on July 11th between 10 and 11 P.M. the curves registered the passage of tremors from an earthquake which was experienced in Central Asia.

Observations with the absolute instruments have been made monthly, and the results are given in the tables forming Appendix I of this Report.

The magnetic instruments have been studied, and a knowledge of their manipulation obtained, by Lieutenant W. J. Combe, R.N., of H.M.S. "Penguin."

With a view of ensuring accuracy in the tables employed in the reduction of magnetic observations, which have been frequently reprinted for use by observers, a careful examination has been made by Professor G. Carey Foster of all the formulæ and blank forms employed in the Kew methods. He has reported to the Committee that he did not detect any inaccuracy.

A careful examination of the observations of Absolute Declination made at Kew from 1858 to the present date has been made at the request of Professors Thorpe and Rücker, who have been investigating the secular changes of Declination during the interval. The results have been forwarded to those gentlemen with the object of their embodiment in the paper on the Magnetic Survey of the British Islands now in progress submitted by them to the Royal Society.

In order to examine certain features of the working of the Kew pattern Vertical Force magnetograph instrument, Mr. Charles Chambers, F.R.S., Director of the Colába Observatory, Bombay, has been provided by the Indian Government with a new instrument of the same pattern, which has been tested at Kew, and forwarded by the maker, Adie, to Bombay.

Dr. van Rijckevorsel, of Rotterdam, visited the Observatory to redetermine the constants of his instruments, and make observations of the magnetic elements prior to the commencement of a new magnetic survey of Holland and Belgium, which he has undertaken under the auspices of the Koninklijke Akademie van Wetenschappen te Amsterdam, so as to connect M. Moureaux's survey of France with Dr. Neumayer's survey of Germany. Dr. van Rijckevorsel selected Kew, Wilhelmshaven, and Paris as his base stations.

Mr. Kitto, Superintendent of the Falmouth Observatory, visited Kew in the spring of the year, to study the methods of constructing tabulating scales for the conversion of the indications of his magnetographs into numerical values, in accordance with the recommendations of the International Commission. He also took advantage of the visit to become conversant with the use of the transit instrument, one of which has been recently acquired and set up at the observatory under his charge.

II. Meteorological Observations.

The several self-recording instruments for the continuous registration respectively of Atmospheric Pressure, Temperature, and Humidity, Wind (direction and velocity), Bright Sunshine, and Rain have been maintained in regular operation throughout the year.

The standard eye observations for the control of the automatic records have been duly registered, together with the daily observations in connexion with the U.S. Signal Service synchronous system. A summary of these observations is given in Appendix II.

The tabulations of the meteorological traces have been regularly made, and these, as well as copies of the eye observations, with notes of weather, cloud, and sunshine have been transmitted to the Meteorological Office.

The readings of the old 100-inch area square rain gauge have been discontinued since February, the new 8-inch circular gauge being now regularly employed, as a check upon the indications of the Beckley self-recording instrument.

The working standard barometer (Newman, 34) of the Observatory, which has been in use continuously since the date of its erection in 1851, having become somewhat worn in its mechanism, was dismounted, and the scale and fittings repaired by Negretti and Zambra, without interfering with the tube and cistern, which were retained at the Observatory. On its return it was again put together and restored to its old place, and fresh comparisons made with the Welsh absolute standards. These showed that a slight shift had taken place in the position of the zero of the scale, a new determination of the scale error was made and fresh corrections accordingly adopted. During the period it was under repair the Royal

Society's old standard barometer was used in the daily observations.

The barograph and thermograph formerly at work at the Armagh Observatory have been put in thorough repair, and set up in the Verification House awaiting the instructions of the Meteorological Council as to their transmission to the new Observatory now erected at Fort William, Inverness, at the base of Ben Nevis. It is the intention of the Committee controlling the Observatory on the summit of that mountain, to maintain a second establishment near the sea-level for the purpose of working in conjunction with it. A Beckley rain gauge has been also provided to complete the equipment.

With the sanction of the Meteorological Council, weekly abstracts of the meteorological results have been regularly forwarded to, and published by 'The Times' and 'The Torquay Directory.' Data have also been supplied to the Council of the Royal Meteorological Society, the editor of 'Symons's Monthly Meteorological Magazine,' Dr. Rowland, and others. The cost of these abstracts is borne by the recipients.

Tables of the monthly values of the rainfall and temperature have been regularly sent to the Meteorological Sub-Committee of the Croydon Microscopical and Natural History Club for publication in their Proceedings. Detailed information of all thunderstorms observed in the neighbourhood during the year has been forwarded to the Royal Meteorological Society soon after their occurrence.

Electrograph.—This instrument has been in constant action throughout the year, and comparisons with the portable electrometer (White, 53) made in March, June, and September show the scale value to have remained unchanged.

III. Solar Observations.

Sketches of Sun-spots have been made on 173 days, and the groups numbered after Schwabe's method, the results being given in Appendix II, Table IV.

Time Signals.—At the suggestion of the Engineer at the General Post Office, a galvanometer has been fitted to the chronograph in order that the Greenwich time signal may be observed on those occasions when it fails to record itself on the chronograph. The 10 A.M. signal has only failed on 16 days throughout the year. On 10 of these days, when it was not received at the usual hour, the later one, at 1 P.M., was duly forwarded by the Post Office. The errors of the Greenwich clock on certain selected dates, when some uncertainty existed as to the correctness of the signal received, have been courteously given after application to the Astronomer Royal.

Transit Observations.—Solar and sidereal transits have been occasionally observed as a check on the signalled times.

Violle's Actinometer.—The Committee have undertaken at the request of the Meteorological Council to make observations with a pair of Violle's actinometers. These consist of two delicate mercurial thermometers encased, the one in a well-blackened hollow metal sphere, the other in the centre of a similar sphere thickly gilded and having a highly polished surface. They have been suitably mounted, and are taken out on sunny days, placed side by side in the open air, and then alternately exposed to the Solar rays, and shielded from its action, the behaviour of the thermometers being noted. Up to the present date, 230 observations have been made with them on seven days.

Solar Physics.—The Committee have handed over to the Solar Physics Committee, with the view of their utilisation, the collection of Solar Negatives from 1858 to 1872 taken at Cranford and at Kew, as well as a large number of undistributed copies of the papers on Solar Physics by Messrs. de la Rue, Stewart, and Loewy.

IV. Experimental Work.

Photo-nephograph.—As it was found that a much more suitable site was offered by the roof of the new building for the working of the cloud cameras, the pedestal was removed from the position it formerly occupied and set up on gratings placed on the new roof, the necessary alterations being effected in the electrical attachments. Opportunity was taken at the same time of replacing, by new wire, about 30 yards of the cable which had become damaged during the building operations. As, however, the question of the most convenient way of utilising the cloud pictures is still under consideration by the Meteorological Council, no photographs have been taken during the past year.

Pendulum Observations.—In November last, the series of pendulum observations at the Observatory, as arranged by General Walker, were successfully carried out, and the apparatus then dismounted and conveyed to the Royal Observatory, Greenwich, where it was set up in the Record Room. Mr. Hollis was instructed by Mr. Constable, the Kew Observer, in the routine of observing in the manner employed at Kew, but the operations had to be postponed for several months owing to a pressure of other work at the Greenwich Observatory. The pendulum swings were commenced in June and are now completed, and the results, at both Kew and Greenwich, are being prepared for publication.

Anemometer Constants.—With a view of examining into the accuracy of the graduation of the small anemometers or air-meters that are very much employed in measuring draughts and air-currents in mine-shafts, galleries, and similar places, a whirling apparatus was roughly constructed with materials at hand and set up in the Optical Room.

By means of this a number of experiments were made, which afforded satisfactory results, with several Lowne's air-meters kindly lent by Mr. Casella, the maker. A more complete whirler has now been constructed, and it is intended to include the examination of these air-meters in the list of operations carried on by the Verification Department.

The electrical anemograph mentioned in the 1886 Report as having been sent to Valencia for erection on that island, was returned to Kew in a somewhat damaged condition after a lengthened trial in a very exposed situation. Certain defects in its construction which became evident during its stay there have now been corrected, and, after undergoing thorough repair, the instrument has been erected on a suitable staging on the roof of the Observatory, with the intention of submitting it to a rigorous comparison with the Beckley anemograph working at the same level about 14 feet due south of it.

V. Verification of Instruments.

The following magnetic instruments have been purchased on commission and their constants determined:—

An inclinometer and unifilar magnetometer for U.S. Navy Department, Washington.

An inclinometer for the University of Modena, Italy.

Three magnets for Mauritius.

The total number of other instruments compared in the past year was as follows:—

Air-meters	3
Anemometers	3
Aneroids	77
Artificial horizons	94
Barometers, Marine	72
,, Standard	63
,, Station	20
Compasses	4
Hydrometers	288
Inclinometers	4
Navy Telescopes	99
,, Binoculars	341
Rain Gauges	15
Sextants	292
,, Shades	42
Sunshine Recorders	2
Carried forward	1419

	Brought forward	1419
Surveyor's Scales		33
Theodolites		5
Thermometers, Arctic		43
"	Avitreous or Immisch's	457
"	Chemical	81
"	Clinical	10116
"	Deep sea	100
"	Meteorological	1910
"	Mountain	28
"	Solar radiation	6
"	Standards	64
Unifilars		4
	Total	14266

Duplicate copies of corrections have been supplied in 26 cases.

The number of instruments rejected on account of excessive error, or which from other causes did not record with sufficient accuracy, was as follows:—

Thermometers, clinical		38
"	ordinary meteorological	16
Various		50

13 Standard Thermometers have also been calibrated, and 6 supplied to different individuals during the year.

There are at present in the Observatory undergoing verification, 10 Barometers, 850 Thermometers, 50 Hydrometers, and 8 Sextants.

The increase in the number of sextants verified during the past year has been considerable, 292 instruments of that kind having been tested, whereas the greatest number in any previous year has been 157. A much larger number of artificial horizons has also passed through the Department, being in nearly all cases flat glass plates set in levelling frames and supplied with spirit levels. These have been all Hall-marked after examination as to trueness of the surface. In accordance with the arrangement mentioned in last year's Report, 440 Navy telescopes and binoculars have been examined and marked for the Admiralty. Also 33 standard measures of length have been tested for the War Office.

The Committee, having considered the advisability of Hall-marking other instruments besides the thermometers submitted to them for verification, instructed Messrs. R. and J. Beck to construct for them an engraving pantagraph, which has been fitted on the Holtzapffel lathe belonging to the Observatory. By its aid the Navy telescopes, binoculars, and Standard Rules have been successfully engraved with mark and number.

VI. Rating of Watches.

During the year 528 entries of watches for rating were made. They were sent for testing in the following classes:—

For class A, 483; class B, 28; and class C, 17.

Of these 119 failed to gain any award; 15 passed with C, 28 with B, 366 with A certificates, and 21 of the latter obtained the highest, class A *especially good.*

In Appendix III will be found statements giving the results of trial of the 26 watches which obtained the highest numbers of marks during the year, the highest position being attained for the third time by Mr. E. F. Ashley. His watch was a keyless single roller fuzee, which obtained 89·1 marks out of a possible 100. He is very closely followed by Mr. A. E. Fridlander, of Coventry, whose keyless double roller fuzee stands only one-tenth of a mark lower on the list, having gained 89·0 marks.

It is satisfactory to note the general improvement in the performance of the watches sent to the Observatory, the proportion of failures of those submitted for trial being but 22 per cent. against 34 per cent. last year. The effect is, moreover, seen in a diminution of the number of entries, for makers are more critical as to the performance of their watches during the timing and springing operations than they were before Kew trials were instituted.

No difficulty has been experienced in maintaining the three safes—in which the watches are placed during rating—at the three temperatures of 40°, 65°, and 90° Faht. respectively, all the year round.

Special attention continues to be given, as before, to the examination of *pocket chronographs*, in accordance with the request of the Cyclists' Union, and the extra tests alluded to in last Report have been regularly enforced.

Marine Chronometers.—Certificates of mean daily rate and of variations of rate at three different temperatures have been awarded to 10 marine chronometers after undergoing the 35 days' trial as specified in the regulations.

VII. Miscellaneous.

Assistance to Observatories, &c.—There have been purchased on commission the following instruments:—Sunshine recorders on Jordan's pattern for the St. Petersburg and Coimbra Observatories; a low range aneroid for Dr. Löwenherz, of Charlottenburg; various pieces of apparatus for the Hong Kong and Mauritius Observatories; and an Ammeter and Voltmeter for Dr. H. Wild, of St. Petersburg.

In accordance with a resolution of the International Meteorological Committee at their Zurich Meeting, a thermometer of very low

range has been constructed to be used as a standard spirit thermometer for comparison with the hydrogen thermometer of the International Office of Weights and Measures at temperatures ranging from zero to about $-70°$ C.

Prepared photographic paper has been procured and supplied to the Observatories at Aberdeen, Batavia, Colába, Falmouth, Lisbon, Mauritius, St. Petersburg, and Stonyhurst, as well as to the Meteorological Office.

Anemograph sheets have been sent to Mauritius, and blank forms for entry of observations, &c., distributed to various applicants.

Old Mural Quadrant.—The Department of Science and Art having accepted the old mural quadrant for exhibition in the science collection at South Kensington, application was made to the Governors of the Armagh Observatory for the telescope and object glass belonging to the instrument, which had been found by Dr. Dreyer in the Armagh collection of astronomical apparatus, forwarded to that Observatory at the time of the abolition of the King's Observatory at Kew in 1840. The Committee's request having been acceded to, the missing parts were duly received at Kew and forwarded to the Museum Galleries at South Kensington.

Exhibition.—The Committee contributed to the annual exhibition of the Royal Meteorological Society, held in March (19–22), several actinometers, solar radiation thermometers, and photometers.

Library.—During the year the library has received as presents the publications of—

29 Scientific Societies and Institutions of Great Britain and Ireland, and

81 Foreign and Colonial Scientific Establishments, as well as numerous private individuals;

The Librarian has been engaged for some time in the preparation of a card catalogue of the library, on the model of that of the Meteorological Office, and has now completed over 1,100 cards, which contain the titles, &c., of all works received by the Committee during the past seven years, together with those of a like title which had been received previously.

The publications not yet catalogued formed part of Sir E. Sabine's Magnetic Office collection, and are chiefly excerpts from foreign publications and reports. They have generally but little interest, and are being examined with the view of binding such as the Observatory does not possess in other forms, and disposing of the duplicates.

Workshop.—The machine tools procured for the use of the Kew Observatory by grants from the Government Grant Fund or the Donation Fund have been duly kept in order.

House, &c.—The external walls of the Observatory, as well as the out-houses, have been thoroughly painted. Book shelves and presses have been fitted to the new rooms, which have also been furnished. Glazed sashes have been inserted in the West Wing Thermometer Room, in place of the panels which formerly filled the window frame, and new pipes have been fixed for the water supply of the House.

Personal Establishment.

The staff employed is as follows:—

G. M. Whipple, B.Sc., Superintendent.
T. W. Baker, Chief Assistant.
H. McLaughlin, Librarian.
E. G. Constable, Observations and Rating.
W. Hugo, Verification Department.
J. Foster " "
T. Gunter.
W. J. Boxall, and seven other Assistants.

(Signed) Francis Galton,
Chairman of the Kew Committee.

November 29th, 1889.

The Kew Observatory. Account of Receipts and Payments for the year ending October 31st, 1889.

Dr. **RECEIPTS.**

	£	s.	d.	£	s.	d.
To Balance from 1887–88	£838	6	10			
Royal Society (Gassiot Trust)	487	10	0			
Meteorological Office (Allowance)	400	0	0			
Experimental Work	99	10	10			
Miscellaneous	79	8	4			
Verifications	841	6	6			
Rating of Watches and Chronometers	465	17	4			
				3211	19	10
Meteorological Office for Postages and Porterages				5	13	9
Commissions executed for Colonial and Foreign Institutions, &c.				328	17	6
				£3546	11	1

PAYMENTS. *Cr.*

	£	s.	d.	£	s.	d.
By Salaries and Extra	£1745	8	0			
Gas, Fuel, Rent, Insurance, &c.	186	1	3			
Stationery, Postages, Porterages, &c.	64	2	9			
Instruments, Apparatus, &c.	97	15	10			
Experimental work	22	2	11			
Miscellaneous	37	10	8			
Verifications	178	11	7			
Rating of Watches and Chronometers	121	18	11			
Pendulum	88	14	10			
				2542	6	9
Extension of the Building and Fittings				88	3	5
Postages and Porterages, &c., for Meteorological Office				5	13	3
Commissions executed for Colonial and Foreign Institutions, &c.				348	1	9
Balance—Bank of England	452	4	2			
London and County Bank	99	18	11			
Cash in hand	10	2	10			
				562	5	11
				£3546	11	1

November 11, 1889. Examined and compared with the Vouchers, and found correct. (Signed) G. CAREY FOSTER, *Auditor.*

ASSETS.

	£	s.	d.
Balance as per Statement	562	5	11
Meteorological Office Allowance, Experimental, and Sundries	36	0	0
Verification Fees due, &c.	129	4	6
Rating of Watches, &c.	48	10	10
Photographic Paper	5	1	3
Commissions, &c.	87	13	11
Blank Forms	28	7	0
Standard Thermometers in Stock	88	0	6
	£985	3	11

LIABILITIES.

	£	s.	d.
To Gas and Sundries	7	12	0
Pendulum Account—Unspent Balance	121	18	0
Purchases on Commission	42	7	11
Balance	813	6	0
	£985	3	11

November 18, 1889. (Signed) G. M. WHIPPLE, *Superintendent.*

APPENDIX I.

Magnetic Observations made at the Kew Observatory, Lat. 51° 28′ 6″ *N. Long.* $0^h\ 1^m\ 15^s{\cdot}1$ *W., for the year October* 1888 *to September* 1889.

The observations of Deflection and Vibration given in the annexed Tables were all made with the Collimator Magnet marked K C 1, and the Kew 9-inch Unifilar Magnetometer by Jones.

The Declination observations have also been made with the same Magnetometer, Collimator Magnet N E being employed for the purpose.

The Dip observations were made with Dip-circle Barrow No. 33, the needles 1 and 2 only being used; these are $3\frac{1}{2}$ inches in length.

The results of the observations of Deflection and Vibration give the values of the Horizontal Force, which, being combined with the Dip observations, furnish the Vertical and Total Forces.

These are expressed in both English and metrical scales—the unit in the first being one foot, one second of mean solar time, and one grain; and in the other one millimetre, one second of time, and one milligramme, the factor for reducing the English to metric values being 0·46108.

By request, the corresponding values in C.G.S. measure are also given.

The value of log π^2K employed in the reduction is 1·64365 at temperature 60° F.

The induction-coefficient μ is 0·000194.

The correction of the magnetic power for temperature t_0 to an adopted standard temperature of 35° F. is

$$0{\cdot}0001194(t_0-35)+0{\cdot}000{,}000{,}213(t_0-35)^2.$$

The true distances between the centres of the deflecting and deflected magnets, when the former is placed at the divisions of the deflection-bar marked 1·0 foot and 1·3 feet, are 1·000075 feet and 1·300097 feet respectively.

The times of vibration given in the Table are each derived from the mean of 14 observations of the time occupied by the magnet in making 100 vibrations, corrections being applied for the torsion-force of the suspension-thread subsequently.

No corrections have been made for rate of chronometer or arc of vibration, these being always very small.

The value of the constant P, employed in the formula of reduction $\frac{m}{X}=\frac{m'}{X'}\left(1-\frac{P}{r_0^{\,2}}\right)$, is $-0{\cdot}00205$.

In each observation of absolute Declination the instrumental readings have been referred to marks made upon the stone obelisk erected 1250 feet north of the Observatory as a meridian mark, the orientation of which, with respect to the Magnetometer, has been carefully determined.

The observations have been made and reduced by Mr. T. W. Baker.

Table I.

Observations of Inclination or Dip.

Month.	Mean Inclination.	Month.	Mean Inclination.
1888.		**1888.**	
October 30......	67° 34′·9	April 26......	67° 35′·1
31......	67 34·4	27......	67 33·0
Mean......	67 34·6	Mean......	67 34·0
November 27......	67 34·0	May 28......	67 34·7
28......	67 34·3	30......	67 32·1
Mean......	67 34·2	31......	67 34·1
December 24......	67 34·2	Mean......	67 33·6
27......	67 34·0	June 24......	67 34·1
Mean......	67 34·1	25......	67 33·8
1889.		Mean......	67 33·9
January 28......	67 34·2	July 29......	67 33·6
29......	67 34·4	30......	67 33·5
Mean......	67 34·3	Mean......	67 33·6
February 25......	67 34·2	August 26......	67 34·2
27......	67 34·7	27......	67 34·9
Mean......	67 34·4	Mean......	67 34·6
March 26......	67 34·0	October 8.....	67 34·7
28......	67 34·6		
Mean......	67 34·3		

Table II.

Observations of the Absolute Measure of Horizontal Force.

Month.	Log $\frac{m}{X}$ mean.	Log mX mean.	Value of m.*
1888.			
November 1st	9·11991	0·30822	0·51768
November 29th	9·11952	0·30828	0·51749
December 28th..........	9·11977	0·30826	0·51763
1889.			
January 30th...........	9·11946	0·30842	0·51754
February 28th	9·11944	0·30839	0·51751
March 29th	9·11925	0·30844	0·51742
April 29th and 30th	9·11919	0·30843	0·51738
May 25th and 27th	9·11876	0·30860	0·51723
June 26th and 27th......	9·11873	0·30857	0·51721
July 31st	9·11846	0·30845	0·51696
August 29th	9·11852	0·30847	0·51701
October 1st and 2nd	9·11830	0·30833	0·51679

Table III.—Solar Diurnal Range of the Kew Declination as derived from selected quiescent days.

Hour.	Summer mean.	Winter mean.	Annual mean.
1889.			
Midnight	−0′·7	−0′·8	−0′·7
1	−0·9	−0·4	−0·6
2	−1·1	−0·4	−0·7
3	−1·2	−0·2	−0·7
4	−1·6	−0·4	−1·0
5	−2·3	−0·5	−1·4
6	−2·9	−0·6	−1·8
7	−3·5	−0·7	−2·1
8	−3·5	−1·0	−2·2
9	−2·7	−1·0	−1·9
10	−0·7	−0·2	−0·5
11	+1·8	+1·2	+1·5
Noon	+4·0	+2·5	+3·2
13	+5·3	+2·9	+4·1
14	+4·7	+2·1	+3·4
15	+3·2	+1·2	+2·2
16	+1·6	+0·5	+1·1
17	+0·6	+0·1	+0·3
18	0·0	−0·1	0·0
19	−0·1	−0·3	−0·2
20	−0·3	−0·6	−0·5
21	−0·3	−0·9	−0·6
22	−0·2	−0·9	−0·6
23	−0·4	−0·9	−0·7

When the sign is + the magnet points to the west of its mean position.

* m = magnetic moment of vibrating magnet.

Table IV.

Month.	Declination.		Magnetic Intensity.								
	Mean of Observations.	Mean corrected for Diurnal Range.	English Units. X, or Horizontal Force.	English Units. Y, or Vertical Force.	English Units. Total Force.	Metric Units. X, or Horizontal Force.	Metric Units. Y, or Vertical Force.	Metric Units. Total Force.	C. G. S. Measure. X, or Horizontal Force.	C. G. S. Measure. Y, or Vertical Force.	C. G. S. Measure. Total Force.
	West.	West.									
1888. October ..	18° 3′·7	17° 58′·1	3·9279	9·5188	10·2975	1·8111	4·3889	4·7480	0·1811	0·4389	0·4748
November .	18 3·1	17 57·7	3·9299	9·5205	10·2996	1·8120	4·3898	4·7490	0·1812	0·4390	0·4749
December .	18 1·2	17 55·8	3·9286	9·5166	10·2956	1·8114	4·3879	4·7471	0·1811	0·4388	0·4747
1889. January ..	18 1·3	17 55·8	3·9308	9·5234	10·3027	1·8124	4·3911	4·7504	0·1812	0·4391	0·4750
February..	18 2·7	17 57·3	3·9308	9·5242	10·3034	1·8124	4·3915	4·7507	0·1812	0·4392	0·4751
March	18 2·3	17 56·8	3·9319	9·5260	10·3055	1·8129	4·3923	4·7517	0·1813	0·4392	0·4752
April	18 6·5	18 1·0	3·9321	9·5242	10·3039	1·8130	4·3915	4·7509	0·1813	0·4392	0·4751
May.. ...	18 7·0	18 1·3	3·9348	9·5275	10·3081	1·8143	4·3930	4·7529	0·1814	0·4393	0·4753
June	18 4·4	17 58·8	3·9348	9·5300	10·3103	1·8143	4·3941	4·7539	0·1814	0·4394	0·4754
July.....	18 7·8	18 2·1	3·9355	9·5293	10·3100	1·8146	4·3938	4·7538	0·1815	0·4394	0·4754
August ..	18 5·8	18 0·4	3·9353	9·5367	10·3169	1·8145	4·3972	4·7570	0·1815	0·4397	0·4757
September.	18 1·5	17 55·5	3·9356	9·5383	10·3183	1·8146	4·3979	4·7576	0·1815	0·4398	0·4758

APPENDIX II.

Meteorological Observations.—Table I.

Mean Monthly results.

Months.	Thermometer.								Barometer.*					Mean vapour-tension.
	Mean.	Means of—			Absolute Extremes.				Mean.	Absolute Extremes.				
		Max.	Min.	Max, and Min.	Max.	Date.	Min.	Date.		Max.	Date.	Min.	Date.	
1888.	°	°	°	°	°	d. h.	°	d. h.	ins.	ins.	d. h.	ins.	d. h.	in.
Oct.….	45·0	53·6	37·5	45·6	66·9	27 2 P.M.	28·1	3 6 A.M.	30·079	30·471	22 10 A.M.	29·290	2 3 P.M.	·251
Nov. …	47·3	51·2	43·3	47·3	58·7	16 Noon	34·9	28 9 „	29·809	30·267	23 6 P.M.	29·219	30 4 A.M.	·280
Dec. …	40·6	44·9	35·6	40·3	57·3	5 1 P.M.	26·1	31 {11 P.M. &Midt.}	30·000	30·523	16 10 A.M.	28·990	22 1 „	·230
1889. Jan.….	37·0	41·0	32·0	36·5	53·0	31 1 P.M.	19·7	6 9 A.M.	30·193	30·751	3 10 „	29·272	10 1 „	·195
Feb. …	37·4	42·5	32·3	37·4	55·8	1 2 „	14·9	13 6 „	29·923	30·476	18 {11 „ &Noon}	29·149	3 8 P.M.	·182
March..	40·6	47·3	34·2	40·8	58·3	24 3 „	20·9	4 7 „	30·000	30·626	15 9 P.M.	28·989	20 4 „	·199
April…	45·8	52·6	39·8	46·2	61·9	{18 5 „ 19 3 „}	33·6	16 5 „	29·751	30·257	19 8 A.M.	29·192	4 3 „	·248
May …	56·0	64·0	48·9	56·5	77·9	24 2 „	41·9	1 Midt.	29·841	30·123	20 9 P.M.	29·490	25 3 A.M.	·360
June…	60·8	69·5	52·6	61·1	79·2	6 3 „	45·8	1 3 A.M.	30·040	30·428	5 8 A.M.	29·634	10 3 „	·402
July …	60·6	68·4	53·5	61·0	76·7	6 4 „	47·0	{19 4 „ 24 5 „}	29·945	30·425	1 11 P.M.	29·553	25 7 P.M.	·402
Aug.….	59·4	67·8	52·1	60·0	80·2	1 1 „	45·1	25 5 „	29·896	30·275	27 10 „	29·209	20 6 A.M.	·399
Sept.….	55·5	63·5	48·1	55·8	76·3	11 4 „	34·7	17 6 „	30·056	30·456	16 9 A.M.	29·518	24 2 P.M.	·350
Means..	48·8	55·5	42·5	49·0	..	….	..	….	29·961	..	….	..	….	·292

The above Table is extracted from the "Hourly Readings," vols. 1888–89, of the Meteorological Office, by permission of the Meteorological Council.

* Reduced to 32° at M.S.L.

Meteorological Observations.—Table II.

Kew Observatory.

Months.	Mean amount of cloud (0=clear, 10=overcast).	Rainfall.* Total.	Rainfall.* Maximum.	Rainfall.* Date.	Weather. Number of days on which were registered: Rain.	Snow.	Hail.	Thunderstorms.	Clear sky.	Overcast sky.	Gales.	Calms.	Wind.† Number of days on which it was: N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Variable.
1888.		in.	in.																		
October ..	5	1·325	0·565	29	8	..	1	..	10	8	..	12	5	2	5	1	2	5	6	4	1
November	8	3·895	0·680	2	19	..	..	..	1	18	1	2	1	2	8	2	3	9	4	..	1
December	7	1·390	0·340	10	10	..	..	..	8	16	..	9	5	1	2	3	8	4	3	3	2
1889.																					
January ..	8	0·910	0·280	9	13	1	..	..	2	20	..	11	10	4	1	1	2	5	5	2	1
February .	8	2·070	0·495	10	16	12	1	..	2	17	..	..	5	5	..	1	1	3	7	4	2
March ...	7	1·360	0·300	7	12	3	..	..	..	16	1	2	4	3	3	2	3	3	8	3	2
April	7	2·235	0·435	9	25	..	2	2	1	16	1	2	6	3	4	2	5	5	2	2	1
May	7	3·045	1·510	26	15	..	2	2	4	12	..	5	3	5	6	1	7	5	3	1	..
June	6	1·280	0·435	15	5	..	1	2	3	10	..	6	5	10	5	1	1	3	2	2	1
July	7	3·050	0·640	12	17	..	2	4	2	12	..	7	2	2	2	2	5	8	5	4	1
August ..	6	2·170	0·335	21	17	..	1	3	5	11	..	3	..	..	1	..	4	15	8	3	..
September	6	1·570	0·795	24	9	..	..	1	6	10	..	6	4	4	3	2	2	6	5	3	1
Totals..		24·300			166	16	9	14	44	166	3	65	50	41	40	18	43	71	58	31	13

* Measured at 10 A.M. daily by gauge 1·75 feet above surface of ground. † As registered by the anemograph.

Meteorological Observations.—Table III.

Kew Observatory.

Months.	Bright Sunshine.				Maximum temperature in sun's rays. (Black bulb *in vacuo*.)			Minimum temperature on the ground.			Horizontal movement of the Air.*		
	Total number of hours recorded.	Mean percentage of possible sunshine.	Greatest daily record.	Date.	Mean.	Highest.	Date.	Mean.	Lowest.	Date.	Average hourly Velocity.	Greatest hourly Velocity.	Date.
1888.	h. m.		h. m.		deg.	deg.		deg.	deg.		miles.	miles.	
October	112 6	33	9 36	1	94	111	27	31	21·0	8	7	27	27
November	26 6	10	6 12	20	70	93	14	39	26·6	28	15	35	7
December	34 48	14	6 0	7	65	96	5	31	18·1	14	9	34	27
1889.													
January	24 0	9	5 36	19	58	87	18	28	19·0	2	8	30	29
February	52 30	19	7 30	15	76	98	18	28	8·8	13	13	33	4, 9
March	83 42	23	8 42	9	92	111	30	28	13·5	4	11	36	21
April	91 42	22	13 12	29	101	120	22, 29	34	22·3	16	11	35	21
May	148 0	31	13 24	24	113	135	24	45	32·4	2	9	32	9
June	199 54	40	13 48	1	121	138	4, 27	49†	40·6	1	8	29	10
July	146 54	30	12 48	6	126	139	13	50	42·3	19	9	29	10
August	169 6	38	12 0	7	123	138	1	48	37·2	25	9	28	20
September	132 36	35	10 24	17	111	131	12	42	26·6	23	8	24	2

* As indicated by a Robinson's anemograph, 70 feet above the general surface of the ground.

† Instrument dismounted for one day.

Table IV.

Summary of Sun-spot Observations made at the Kew Observatory.

Months.	Days of observation.	Number of new groups enumerated.	Days apparently without spots.
1888.			
October	21	1	19
November	7	2	1
December	7	1	3
1889.			
January	10	6	10
February	8	2	3
March	18	1	11
April	14	1	11
May	19	1	17
June	21	1	10
July	19	5	8
August	14	3	3
September	15	1	10
Totals	173	19	106

APPENDIX III.—Table I.

Results of Watch Trials. Performance of the 26 Watches which obtained the highest number of marks during the year.

Watch deposited by	Number of watch.	Balance spring, escapement, &c.	Mean daily rate. — +Gaining. −Losing.	Mean variation of daily rate. ±	Mean change of rate for 1° F	Difference of mean daily rate: Between pendant up and dial up.	Between pendant up and pendant right.	Between pendant up and pendant left.	Between dial up and dial down.	Difference between extreme gaining and losing rates.	Marks awarded for: Daily variation of rate.	Change of rate with change of position.	Temperature compensation.	Total Marks. 0—100.
			secs.	secs	secs.	secs.	secs.	secs.	secs.	secs.				
E. F. Ashley, London	03801	Single overcoil, *s.r., fusee	+1·4	0·35	0·04	−0·1	−0·2	−0·6	−0·6	4·25	33·0	38·9	17·2	89·1†
A. E. Fridlander, Coventry	52292	Single overcoil, *d.r., fusee	+4·9	0·4	0·01	−0·8	−0·1	−1·1	−0·9	4·0	32·1	37·7	19·2	89·0†
E. F. Ashley, London	03770	Single overcoil, s.r., *g.b.	−0·9	0·4	0·06	+1·0	+0·3	0·0	−0·1	3·75	32·2	38·3	16·1	86·6†
W. Holland, Rock Ferry	3612	Single overcoil, d.r., fusee, centre-seconds	+0·9	0·4	0·05	0·0	+0·1	+0·5	+1·5	5·0	31·6	38·2	16·5	86·3†
Stauffer, Son, & Co., London	126681	Single overcoil, d.r., g.b., bar-lever	−7·8	0·4	0·05	+0·8	−2·2	+0·9	−1·5	6·25	31·8	36·1	16·8	84·7†
Joseph White, Coventry	32133	Single overcoil, d.r., g.b.	−4·7	0·4	0·04	−0·7	−1·8	+1·2	+2·8	5·25	32·2	35·2	17·1	84·5†
Baume & Co., London	2836	Single overcoil, d.r., g.b., bar-lever	+1·0	0·5	0·04	+1·2	+0·8	−0·8	−2·4	4·5	30·6	36·5	17·3	84·4†
Usher & Cole, London	25430	Single overcoil, s.r., g.b.	+0·3	0·5	0·06	−0·9	−0·3	−0·7	+2·1	6·75	30·2	37·6	16·3	84·1†
Rotherham & Sons, Coventry	84377	Single overcoil, s.r., g.b	+2·9	0·4	0·05	+4·6	+0·8	+1·0	−3·2	7·0	31·8	35·1	16·9	83·8
J. J. Stockall, London	10416	Single overcoil (palladium), s.r., g.b.	−1·9	0·4	0·03	−2·7	+3·1	+0·5	+1·8	8·0	31·7	34·2	17·9	83·8
Rotherham & Sons, Coventry	84278	Single overcoil (palladium), s.r., g.b.	+2·3	0·5	0·03	0·0	−0·9	−1·6	+3·2	6·25	30·6	35·1	18·0	83·7†
Stauffer, Son, & Co., London	124228	Single overcoil, d.r., g.b.	+2·8	0·4	0·04	−2·0	+1·3	+3·0	+1·7	8·0	32·0	34·4	17·2	83·6†
Joseph White, Coventry	31614	Single overcoil, d.r., g.b., centre-seconds	+0·1	0·5	0·04	+1·4	0·0	+2·5	−0·5	4·75	29·3	36·8	17·4	83·5†
H. Golay, London	2808	Double overcoil, d.r., g.b.	+2·8	0·5	0·03	−2·3	+1·2	−2·3	+0·3	7·25	30·6	34·6	17·9	83·1†
Baume & Co., London	2786	Single overcoil, d.r., g.b., bar-lever	+1·3	0·4	0·05	+1·2	−1·8	−0·6	−4·0	5·25	31·2	35·2	16·5	82·9
W. Holland, Rock Ferry	3641	Single overcoil, d.r., g b., centre-seconds	+0·3	0·4	0·07	+0·6	−2·9	−1·9	−2·5	5·5	31·6	35·3	15·2	82·1†
W. Holland, Rock Ferry	3247	Single overcoil, d.r., fusee	+0·1	0·6	0·01	−1·0	+1·6	+1·9	−0·1	6·5	27·6	35·3	19·1	82·0
Joseph White, Coventry	31812	Single overcoil, d.r., g.b.	+2·8	0·6	0·06	−0·8	−0·9	−1·3	+1·0	6·0	28·4	37·8	15·8	82·0
A. E. Fridlander, Coventry	52625	Single overcoil, d.r., g.b.	+2·2	0·4	0·07	−0·1	+2·4	+2·8	−0·7	6·25	31·9	34·5	15·1	81·5†
Gordon & Sons, Kingston-on-Thames	55781	Single overcoil (palladium), d.r., g.b.	+1·9	0·5	0·07	−1·8	−1·4	−3·9	−0·4	7·25	29·9	36·2	15·3	81·4
E. F. Ashley, London	03784	Single overcoil, d.r., fusee	−0·6	0·4	0·10	+1·5	−1·0	−0·6	−0·4	6·25	31·1	36·5	13·5	81·1†
Stauffer, Son, & Co., London	124225	Single overcoil, d.r., g.b.	+4·2	0·6	0·07	+1·0	+1·0	+1·0	−0·5	6·25	27·4	38·6	15·1	81·1
A. E. Fridlander, Coventry	52498	Single overcoil, d.r., g.b.	+2·9	0·5	0·05	−0·6	−2·1	−5·2	−0·5	8·0	30·4	34·0	16·7	81·1
A. E. Fridlander, Coventry	52483	Double overcoil, d.r., g.b.	−1·9	0·4	0·04	−1·9	+3·7	−4·6	+1·3	0·0	32·0	31·9	17·1	81·0†
Baume & Co., London	2778	Single overcoil, d r., g.b., bar-lever	−1·7	0·5	0·07	+0·3	−0·6	+1·4	−3·1	16·25	29·8	35·6	15·6	81·0
L. Rozat, Chaux-de-Fonds	42102	Single overcoil, s.r., g.b.	+6·1	0·7	0·04	−0·1	+1·0	−0·1	+0·9	5·75	25·8	38·1	17·1	81·0

* d.r., double-roller; s.r. single-roller; g.b., going barrel. † Especially good.

APPENDIX III.—Table II.

Highest Records obtained by Complicated Watches during the year.

Description of watch.	Number.	Deposited by	Marks awarded for			Total marks, 0—100.
			Variation.	Position.	Temperature.	
Minute and seconds chronograph and repeater ..	52625	A. E. Fridlander, Coventry ...	31·9	34·5	15·1	81·5*
" " " " ..	14781	S. Smith and Son, London ...	27·0	28·6	9·1	64·7
Split-seconds and minute-recorder chronograph.	2836	Baume and Co., London......	30·6	36·5	17·3	84·4*
" " " " .	2813	H. Golay, "	26·7	33·8	15·5	76·0
" " " " .	125935	Stauffer and Co., "	25·2	35·0	14·2	74·4
Minute and seconds chronograph...........	52483	A. E. Fridlander, Coventry ...	32·0	31·9	17·1	81·0*
" " "	124440	Stauffer and Co., London	30·2	32·8	17·9	80·9
" " "	3678	H. Golay, "	27·5	35·7	17·0	80·2
Repeater	1703	W. Vasel, London	30·7	35·3	10·2	76·2*
"	30388	M. F. Dent, "	26·7	31·2	15·8	73·7
"	3677	H. Golay, "	28·7	29·1	13·4	71·2
Ordinary seconds chronograph	30822	S. Smith and Son, London....	25·1	28·4	9·7	63·2
" " "	99771	Newsome and Co., Coventry ..	23·6	28·7	10·4	62·7

* Especially good.

APPENDIX IV.

List of Instruments, Apparatus, &c., the Property of the Kew Committee, at the present date out of the custody of the Superintendent, on Loan.

To whom lent.	Articles.	Date of loan.
G. J. Symons, F.R.S.	Portable Transit Instrument	1869
The Science and Art Department, South Kensington.	The articles specified in the list in the Annual Report for 1876, with the exception of the Photo-Heliograph, Pendulum Apparatus, Dip-Circle, Unifilar, and Hodgkinson's Actinometer.	1876
Lieutenant A. Gordon, R.N.	Unifilar Magnetometer by Jones, No. 102, complete, with three Magnets and Deflection Bar. Dip-Circle, by Barrow, one Pair of Needles, and Magnetizing Bars. One Bifilar Magnetometer. One Declinometer. Two Tripod Stands.	1883
Professor W. Grylls Adams, F.R.S.	Unifilar Magnetometer, by Jones, No. 101, complete. Pair 9-inch Dip-Needles with Bar Magnets	1883 1887
Professor O.J. Lodge, F.R.S.	Unifilar Magnetometer, by Jones, No. 106, complete. Barrow Dip-Circle, No. 23, with two Needles, and Magnetizing Bars. Tripod Stand.	1883
Captain W. de W. Abney, F.R.S.	Mason's Hygrometer, by Jones	1885
Prof. T. E. Thorpe, F.R.S.	Tripod Stand	1886
Lord Rayleigh, F.R.S.	Standard Barometer (Adie, No. 655)	1885

NOTICES TO FELLOWS OF THE ROYAL SOCIETY.

A Monograph of the Horny Sponges, by Dr. R. von Lendenfeld, is now published (see above). The price to the public is £3; to Fellows of the Royal Society, £2. As the edition is limited to 500 copies, Fellows should order their copies at once of the Assistant Secretary.

The following are now offered *to Fellows only* at reduced prices:—

"Catalogue of the Scientific Books in the Library of the Royal Society"—

Part 1.	Transactions, Journals, &c......	4*s*. net.
Part 2.	General Catalogue............	7*s*. ,,
The two Parts	....	10*s*. ,,

"International Polar Expedition. Fort Rae Observations." 10*s*. 6*d*. net.

"Catalogue of Scientific Papers," in sets:—

	Net prices. £	*s*.	*d*.	
Vols. I—VI in Half-Morocco at	3	10	0	the set.
*Vol. I, Half-Morocco, and II—VI, Cloth. .	2	14	6	,,
Vols. VII, VIII in Half-Morocco at . .	1	10	0	the two.
,, ,, ,, in Cloth	1	1	0	,,

The Library is now open from 11 A.M. to 6 P.M. (Saturdays 1 P.M.).

Philosophical Transactions.—The Fellows are now permitted to give standing orders for the delivery of the volumes of 'Philosophical Transactions' to their addresses as published. The order must be in writing, and should be sent to the Assistant Secretary.

On application to Messrs. Harrison and Sons, 46, St. Martin's Lane, Fellows and others can have the separate numbers of the 'Philosophical Transactions' bound in cloth case for 2*s*. 6*d*., or can obtain cloth cases for binding, price 1*s*. 6*d*.

List of Fellows.—Fellows who desire to have the Annual List posted to them every year as soon as issued should send their names to the Assistant Secretary.

A printed post-card of the papers to be read at each meeting will be sent weekly to any Fellow upon application to Messrs. Harrison and Sons, 46, St. Martin's Lane, W.C.

* Vol. I in cloth is sold out.

HARRISON AND SONS, 45 & 46, ST. MARTIN'S LANE, W.C.,

AND ALL BOOKSELLERS.

PROCEEDINGS OF

THE ROYAL SOCIETY.

VOL. XLVI. No. 285.

CONTENTS.

Price Three Shillings and Sixpence.

FEB. 13, 1890.

APPENDIX IV.

List of Instruments, Apparatus, &c., the Property of the Kew Committee, at the present date out of the custody of the Superintendent, on Loan.

To whom lent.	Articles.	Date of loan.
G. J. Symons, F.R.S.	Portable Transit Instrument................	1869
The Science and Art Department, South Kensington.	The articles specified in the list in the Annual Report for 1876, with the exception of the Photo-Heliograph, Pendulum Apparatus, Dip-Circle, Unifilar, and Hodgkinson's Actinometer.	1876
Lieutenant A. Gordon, R.N.	Unifilar Magnetometer by Jones, No. 102, complete, with three Magnets and Deflection Bar. Dip-Circle, by Barrow, one Pair of Needles, and Magnetizing Bars. One Bifilar Magnetometer. One Declinometer. Two Tripod Stands.	1883
Professor W. Grylls Adams, F.R.S.	Unifilar Magnetometer, by Jones, No. 101, complete.	1883
	Pair 9-inch Dip-Needles with Bar Magnets ...	1887
Professor O. J. Lodge, F.R.S.	Unifilar Magnetometer, by Jones, No. 106, complete. Barrow Dip-Circle, No. 23, with two Needles, and Magnetizing Bars. Tripod Stand.	1883
Captain W. de W. Abney, F.R.S.	Mason's Hygrometer, by Jones	1885
Prof. T. E. Thorpe, F.R.S.	Tripod Stand..........................	1886
Lord Rayleigh, F.R.S.	Standard Barometer (Adie, No. 655)	1885

"On the Velocity of Transmission through Sea-water of Disturbances of large Amplitude caused by Explosions."

By RICHARD THRELFALL, M.A., Professor of Physics, University of Sydney, and JOHN FREDERICK ADAIR, M.A., Demonstrator of Physics in the University of Sydney. Communicated by Professor J. J. THOMSON, F.R.S. Received March 14, 1889. Read March 21, 1889.

[PLATES 3, 4.]

PART I.—EXPERIMENTAL.

During the latter months of the year 1886 and the first half of the year 1887, experiments were undertaken by one of us in conjunction with Lieutenant Ottley, R.N., of Her Majesty's ship "Nelson," then stationed at Sydney, with the view of obtaining definite information as to the existence or non-existence of the supposed phenomenon of "explosion by influence." It had been previously discovered (Threlfall, 'Phil. Mag.,' March, 1886) that in the case of small explosions of fulminate of mercury under water, there was a sort of directed effect due to want of symmetry in the disposition of obstacles surrounding the centre of explosion. This suggested that a possible explanation of the recorded cases of explosion by influence might be found by a careful examination of the initial conditions. Since in ordinary torpedo work in which the initial conditions of explosion are greatly varied, cases of sympathetic explosion are generally believed to have occurred, it was considered that the most hopeful way of proceeding would be by endeavouring to obtain sympathetic explosions between masses of explosive, fired under such conditions as to preclude the possibility of directed action. To this end small charges of guncotton were suspended in free air by strings, the distance between the charges was measured, and one of them was detonated. Out of about fifty experiments of this kind we did not succeed in obtaining any explosion by influence, though the target charge was generally blown to atoms. A similar series of experiments, in which "9-oz." disks of guncotton were wrapped in oil-silk and exploded under water, resulted in the same conclusion. Explosion of the target charge was only brought about when so close to the exploding charge as to be exposed to explosive violence amply sufficient to account for the explosion. In some cases the conditions were varied by placing detonators in the target charge, but no exception to the general result was noticed. At this time we succeeded in obtaining a copy of Major-General Abbott's 'Submarine Mines and Explosives,' and there found that the directed action had been noticed by him in the case of explosive mixtures in a case with an

air-space at one end (p. 66), and in the case of dynamite (p. 82) in a "tin case, with saw-dust tamping" (Appendix B, p. 433). The sympathetic explosion of dynamite and guncotton had also been investigated by General Abbott, with the general result (pp. 122 *et seq.*) that as a distinct mode of explosion it does not exist.

As this conclusion agrees entirely with the one to which we were led, both with explosions by guncotton in air and water, and explosions of guncotton, gelatine-dynamite, dynamite No. 1, and diazobenzene nitrate on one side of a large iron plate, the target charge being on the other side, we did not pursue the matter further. We may remark that we found diazobenzene nitrate a very good substance to experiment on in this direction, as it is exceedingly sensitive, but not so violent as silver fulminate. Like the latter and iodide of nitrogen, we believe that its explosion can be caused by the friction of the small crystals of which it is composed on one another. If two heaps of this substance, placed on a thin iron plate, were protected by cardboard cylinders without ends, resting on the plate supporting the explosive and surrounding the latter, explosion always took place at a greater distance when the supporting plate was sheet iron than when it was a rather thicker pine board. Owing to the thinness of the iron plate, the amplitude of the vibrations into which it was thrown was greater than in the case of wood, and consequently the target heap was more thrown about, and exploded (other things being equal) at a greater distance. We have to thank Dr. Helms, Demonstrator in Chemistry at the University, for his kindness in preparing the diazobenzene nitrate for us; it is both tiresome to make and delicate to handle.

On page 433 of his 'Submarine Mines and Explosives,' General Abbott gives in an appendix an account of a remarkable explosion of 5 lb. of dynamite. In this experiment the axis of maximum effect was bent out of the vertical (*i.e.*, the normal to the water-surface through the shot) to such an extent as to call for special comment. A theory had been proposed by one of us which we thought might account for this abnormal result, if it could be shown that it was only an extreme case of a generally occurring phenomenon. With the view of testing this theory, we decided that the best way would be to find the velocity of transmission of explosive energy through water, varying the quantity and nature of the explosive and its case, and noting any irregularity that might occur. This, after two years' work, we have succeeded in doing, extending our observations to guncotton and Nobel's No. 1 dynamite, and varying the quantity of explosive from 9 oz. up to 5 lb. charges. The distance over which the velocity was measured was varied three times, from about 170, the least, to 200 yards, the greatest distance. The results have been such as to justify the title of this paper.

The Experiments.

The Site and General Arrangements.

Through the kindness of the military authorities we were allowed to use the torpedo station at Berry's Bay, in the harbour of Port Jackson, as the base of operations. This bay had the advantage of deep water close to the shore, of being comparatively quiet, and within about an hour's journey of the University. The Sydney Marine Board with great courtesy allowed us to use their steam-launches for the purpose of getting to the ground, it being out of the course of the ferry-boats. The general plan of the water and coast-line will be understood from an examination of the chart (Plate 3) kindly prepared for us by Mr. R. J. A. Roberts, who also undertook the surveying of the distance between the observing stations. We desire to express our indebtedness to Mr. Roberts, for his very accurate determinations to be referred to hereafter. The general disposition of the arrangements was as follows:—Two piles, each about 45 feet long, were driven into the mud of the sea-bottom at the stated distance apart, and about 40 yards from the shore. To these piles were attached the apparatus for recording the arrival of the disturbance due to the explosions; the connexions with the shore were made by means of two lengths of armoured cable, kindly lent to us by the officer commanding the torpedo corps. At first we tried to use overhead wires supported on telegraph poles, and running from the piles to the shore; these, however, were so systematically carried away by passing steamers, that they had to be abandoned.

In a portable constructor's house on the wharf we erected the pendulum chronograph, to be described shortly, and there established the arrangements for firing the charges and working the recording apparatus.

About a year was spent in experimenting with different forms of apparatus for recording the arrival of the disturbance. Three forms of "gauge" were devised, tested, and abandoned before the ultimate form was hit upon. Considerable difficulty was also experienced in avoiding missfires, as we desired to avoid complicating matters by using heavy cases for our torpedoes. At one time work was entirely suspended for three months, as we could not get any guncotton, and were not allowed to store the dynamite which we could buy in the market; guncotton cannot be procured here except through courtesy of the Government. The same remark applies to detonators, which we were forced to devise a means of manufacturing for ourselves. Finally, owing to the good offices of Major Penrose, R.E., we obtained a supply of 100 lb. of guncotton from the Government, and several boxes of detonators, for which we desire to record our thanks. In our opinion any work of the kind should not in the future be under-

taken except by such military officers as are in a position to avoid the imposition of difficulties about the obtaining of explosives.

The general course of conducting an experiment was as follows :—

The gauges were got out, adjusted to their maximum sensitiveness, and generally overhauled.

The whole number of torpedoes for the day's work were charged and primed.

The gauges were taken out in a dinghy, and adjusted to their supports on the piles by one observer. The electrical circuits were completed and tested, and the firing wires were run out off drums, and the ends buoyed at about 30 yards outside each pile, and in the direct line of the two piles.

The temperature of the water was taken at the surface, and at a depth of 6 feet.

Meanwhile the other observer got the chronograph adjusted, noted the temperature of the tuning-fork, assured himself of the perfection of the electrical firing arrangements, and smoked the glass plates. All being ready, one observer (J. F. A.) generally assisted by Mr. Proctor, the torpedo storekeeper, shipped a torpedo, and proceeded to the firing point, and there connected it with the firing wire in the usual manner.

The other observer (R. T.) made the final arrangements in the chronograph hut, and saw that all the precautions to prevent premature explosion were observed. On receiving a signal to the effect that the torpedo was ready, he replied by a signal to drop the shot and run away. The end of the firing cable, previously carefully insulated, was then connected to a wire passing into the hut. The plates of the bichromate firing battery were then lowered into the liquid, and a safety-plug was placed in the firing position. At a signal from the party in the boat, the pendulum was allowed to fall, firing the torpedo, and carrying the recording plate against the scribers connected with the recording apparatus and the scriber of the tuning fork. Immediately after firing, the trigger was reset, the safety-plug placed in its second position, the battery plates lifted out of the solution, and the cable insulated. It will be seen that no less than four separate and independent acts of precaution were thus performed after each shot. We have had no accidents of the nature of premature explosions after several hundred shots, though once in boring a dirty guncotton disk with a brass drill, a partial explosion, happily without doing any damage, took place.

After the firing of the shot the party in the boat proceeded to row past the other pile, and there fastened the next shot to its appropriate buoyed cable. Meanwhile the observer on shore took the smoked plate out of the chronograph, wrote on it whatever data were necessary, allowed the pendulum to swing back, readjusted the scribers

and tuning-fork, replaced the smoked plate, and waited for the signal as before.

The operations were then repeated till half the prescribed shots had been fired; the gauges were then reversed on the piles, and the same process again gone through. The greatest number of *records* we ever succeeded in obtaining in a single day was sixteen. The greater part of the time was taken up in adjusting the gauges and preparing the charges. At the end of the day's work the gauges were brought in, the firing wires wound up, and the plates packed in a box for measurement at the University. Four complete records were obtained on each plate. Shots were fired alternately from left to right, and from right to left, past the piles and gauges, with a view to the elimination of the time-constants of the recording apparatus.

We have therefore the following experimental matters to explain:—

1st. The measurement of the time of passage of the shock through water, from one gauge to the other.

2nd. The measurement of the distance between the gauges.

3rd. The precautions requisite to ensure symmetry of explosion, and of explosion in the line joining the two gauges.

We shall consider the time-measurements first. In deciding on a chronograph we had to bear in mind that it was an essential condition of success to get an instrument which allowed of rapid and easy manipulation. Since we required to make absolute measurements, that is, to be able to reduce our chronograph indications to mean solar seconds, it follows that some standard time measurer must be adopted, even at the expense of otherwise reducing the handiness of the apparatus, and of running the risk of a diminished sensitiveness. It is a truism that the methods which give the most absolute values are seldom as sensitive as those which allow of a slight risk of error in the absolute value of their indications. Considerations such as these led to the preference being given to an instrument of the falling pendulum or myograph description. Such an instrument was therefore commenced in January, 1887, by Mr. Cook, the University Assistant in the Physical Laboratory, though at the time his appliances were of the most meagre description. The pendulum was about a yard long, the bob consisting of a flat oval mass of lead. This carried the supporting and adjusting screws for the glass plate. There were holdfast catches for the bob at each end of its swing. On the base of the instrument there were three separate appliances. First, a stand to support the tuning-fork, and allow of its easy adjustment in a vertical plane, and in a plane parallel to the plane of motion of the pendulum. The plane of the latter was invariable, as it swung on hard steel knife-edges, supported on shallow grooves cut in steel plates.

The tuning-fork—one of Koenig's—making, or rather marked as giving, a hundred vibrations per second, was maintained electrically by a second battery. The scribing apparatus, consisting of two scribers and two electromagnets, could be adjusted through a considerable range in a vertical plane, and in a plane perpendicular to the motion of the pendulum; and also to some extent in a plane parallel to the plane of motion of the pendulum. There was a good deal of trouble about this part of the apparatus, chiefly resulting from the desire we had to lose no sensitiveness through slowness of action. To this end the levers of the scribers were made of aluminium, and the magnets and springs were very powerful as compared with the moment of inertia of the scribers around their axes. We altered this apparatus, almost remaking it several times, as we found that the limit to the accuracy of our time observations lay in the discrimination of the precise point at which the straight line, traced by the scriber at rest on the smoked glass, began to be interrupted by the motion of the scriber. The sharper the bend the more accurate the discrimination. Finally we got the scribers to break the line with almost complete suddenness, *i.e.*, at something like 90° to its original direction.

The trigger for firing the charge was of a kind that will easily occur to the reader. The catch on the pendulum could be slightly set forwards or backwards, and only affected the trigger when the pendulum was moving in one direction. The trigger itself could be moved along on an arc below the pendulum, so as to allow the firing to take place at such a time as would give the scribers the benefit of the pendulum's most rapid motion. In order to ensure a good instantaneous contact, the trigger on being let off drove (under the influence of a powerful spring) a platinum-coated wedge between the opposing faces of the brass interrupting plugs. The firing battery consisted of six good bichromate elements with large plates; and the battery for the recording gear consisted of twenty Leclanché cells, ten with large plates and ten with small. In all cases use was made of a good earth "return," this being secured permanently at one end with a bit of armoured cable, at the piles with copper plates, and at the torpedo with a bit of old copper wire or thin plate.

Receiving Apparatus.—" Gauges."

This part of the experimental arrangement was the most difficult to get to work satisfactorily. At first a sort of small pendulum was supported so as to hang near an india-rubber disk, inside a wide brass cylinder; the whole was sunk under water, leaving the contact wires protruding. The difficulties, however, were such as to render this form of apparatus useless. The form finally adopted for very small explosions, mere noises, in fact, is shown in Plate 4. The great

trumpet ends in a small tube, closed with an india-rubber film diaphragm. Against this diaphragm rests an independently supported strip of aluminium foil, as fine and light as possible; at its end it carries a tiny point of platinum. The fine diaphragm, with its tube and aluminium contact, is protected by an arrangement of flanges carrying a glass cylinder, with stout brass ends. Through one brass end passes a long brass screw, carrying a platinum disk at its lowest point. This disk is insulated from the brass portion by means of ebonite, and is connected by a wire with an insulated binding screw outside the apparatus. The screw is adjusted so that it just clears the platinum point on the aluminium spring. If the diaphragm is disturbed the point is thrown against the disk and makes contact. In order that the contact may be quite certain, the end of the platinum wire must be cut off quite clean, and the platinum disk carefully cleaned. The contact obtained by means of a platinum wire with a fused end is useless, as its definition is more a matter of pressure than anything else. As the apparatus has to be sunk so that its lower end goes to a depth of 5 or 6 feet in water, it is of course necessary to pump in air to distend the india-rubber, closing the mouth of the trumpet, against the water-pressure. This implies the use of a simple pressure gauge, and will also explain the necessity for the air-tight glass chamber in front of the fine rubber diaphragm. In order that the pressure on both sides of this diaphragm may be normally the same, a "leak" is provided, running from the tube supporting the diaphragm to the space in front of it. This "leak" consists of a bit of drawn-out thermometer tubing, about an inch long.

The apparatus with which all the earlier experiments were performed is only suitable for explosions of from 6 or 7 oz. of guncotton. In this apparatus the big trumpet is replaced by a drum connected with the upper portion of the apparatus by means of a brass tube having an internal diameter of nearly three-quarters of an inch. The diameter of the drum was 6 inches, and each drum-head consisted of an india-rubber diaphragm, so that each gauge had two india-rubber faces. In order to prevent the india-rubber cutting at the edge of the drum, the latter must be finely polished round the rim, and this should, if possible, be made of round brass wire. We found that the decay of the india-rubber was lessened by lubricating the rim of the drum with tallow. By far the best thing to use for binding the india-rubber on to the drums is silk fishing-line.

Both kinds of gauges were fixed to substantial wooden supports, and these were provided with copper or iron cleats to run in guides on a thick plank spiked on to the "turpentine" pile. The drum gauges were sunk to the required distance by means of lead sinkers; the trumpet gauges required to be hauled under water by a block and tackle attached to a 5-cwt. sinker dropped at the foot

of the pile. The handling of these big trumpets required several men; even the smaller gauges were most troublesome to fix if the weather was bad. The chief difficulty met with in the earlier forms of gauge was their liability to leak; in the later forms this was got over by the method of tying on the india-rubber and by cementing in the glass cylinder between two concentric rings of brass with melted sealing-wax. Glue mixed with bichromate of potash and melted in the dark is also an excellent cement, if coated with shellac varnish. The screw carrying the platinum disk passed through a stuffing box filled with an india-rubber bung, which could be jammed tight by means of a screw top. During the course of the experiments it was constantly necessary to adjust the depth of the gauges to allow for the rise and fall of the tide.

In the experiments, the results of which are given in this paper, the "drum" gauges were exclusively employed. This is due to two causes. In the first place, we seldom commanded sufficient manual assistance to work the trumpets properly; and in the second, these latter were so sensitive to the action of small waves on the surface of the water that we could not utilise their full sensitiveness for the purpose of registering the explosions. Except on some few days in the course of the winter, the requisite calmness of the water is not attained in Port Jackson. It may be mentioned that it is easily possible to set these large gauges to such a point of sensitiveness that they will make contact on receiving the verbal order to do so—if it is uttered in the right tone from a distance of 10 or 15 yards.

Appliances connected with the Firing of the Charges.

The charges themselves were always enclosed in thin tin cylinders, or else in oil-silk or mackintosh. Except with respect to the detonators, there is little to say. When we were driven to make them for ourselves, we finally, with the assistance of Mr. Cook, hit on the following method. A wooden mould was prepared, of such a form that it allowed a cylinder of plaster of Paris to be cast round two lengths of gutta-percha-covered copper wire running parallel to each other and to the long axis of the cylinder. When the plaster had set, the wires were cut off at one end to within about half a millimetre of the end surface of the cylinder, and at the other were allowed to protrude to a distance of 5 or 6 inches. The fusible bridges of thin platinum wire were secured to the short ends of the copper wire by solder and resin only. These bridges were primed with a good covering of a mixture of guncotton fluff and meal powder, made into a paste with collodion. In order to attach the detonating cap, advantage was taken of the good workmanship of ordinary cartridge cases. A "number twelve bore" case was taken and cut off above the brass

work in the lathe. One end of the cardboard cylinder thus obtained was closed by having a wooden plug glued into it. Through this plug a hole was drilled to fit the detonating caps we were able to buy. A cap was pushed, open end first, into the hole till it was flush with the inner surface of the plug; it was secured in this position with the help of shellac varnish. The empty portion of the cap was then filled with a mixture of fulminate of mercury and guncotton fluff, and this filling was allowed to cover the bottom of the cartridge case above the plug. Finally, some bees'-wax, resin, and linseed-oil luting was poured over the surface of the upper portion of one of the plaster cylinders, and this was then pushed home, so that the priming rested on the loose mixture above the plug.

Finally, the whole thing was dipped into some of the same luting, so as to give it a waterproof coat. The detonators were then tested with a telephone and stored.

The advantage of this method is that as each operation is quite simple the detonators can be formed by unskilled labour; a consideration, if many have to be made. The luting must have some linseed oil with it, otherwise it is apt to crack in cold weather. We have never known a detonator so constructed to miss fire, though missfires often occurred till we used the fulminate and guncotton priming. The collodion paste is also very suitable, as it protects the delicate bridge. In most cases the charges to be fired were contained in tin cases, big enough to hold one 9-oz. disk of guncotton. The lid of each tin had a cylindrical tin tube projecting from it to a distance of 4 inches; this tube was not central, because 9-oz. disks are bored with two holes. The detonators were secured in these tubes simply by dipping the tubes in hot luting and then pushing the detonators down till the cap projected to a sufficient distance.

The charge was then placed in the tin, the detonator passing into one of the holes of the cotton, and then the junction between the lid and the tin was made secure with luting. For smaller charges than 9 oz. other receptacles were provided; in most cases simply wrapping the charge in mackintosh cloth was sufficient to keep out the water long enough for our purpose, the joints being well secured with a solution of india-rubber. One terminal of the detonators was always put to earth.

Apparatus for the Measurement of the Plates.

As far as we can learn, it has hitherto been usual in the interpretation of the markings on a smoked plate simply to count the whole number of fork-beats, and to estimate the fractions at best by means of a pair of spring dividers. In our case this method is obviously not on a par, as far as accuracy goes, with the other measurements.

Besides this, the time is estimated by the distance between the breaks, not in one line, but in two lines separated by about 3 mm., the fork-line being about 8 cm. below the lowest of the scribing lines. It was clear at the outset, therefore, that special arrangements must be made if real accuracy was to be looked for. This we have attained in the following manner:—

By the construction of the myograph, and especially of the scribers, it is possible to set the two scribing points and the scribing point of the fork in the same vertical line. Now, by proper adjustment, this line is brought to coincide with a line passing through the knife-edges and the centre of gravity of the smoked surface of the rectangular glass plate when the pendulum is at rest. The three points, therefore, at any instant during the motion of the pendulum, lie on a radial line passing through the centre of suspension of the pendulum. If, therefore, proper radial lines are drawn on the plates, corresponding to any epoch marked by the scriber, these lines will cut the "fork" line at the point corresponding to the same epoch. In fact, the object is to find the exact point on the plate which was in contact with the fork at the instant that the scriber concerned began to move.

We had a scribing table made to find these points in the following manner. A crossed board was prepared, about a foot longer than the distance from the knife-edges of the pendulum to the bottom of the glass plate. A ground axle-pin was provided, passing through the board perpendicular to its plane near one end, and firmly fastened to it by brass collars. A long brass rod of rectangular section was furnished with a projection on one side at one end, and through this projection was bored the hole into which the axle-pin was ground. The brass rod could therefore revolve on the board about a point situated at a distance of about three-quarters of an inch from one of its shorter edges. The distance between the knife-edges of the pendulum and the centre of the glass plate was carefully measured, and a recess was cut in the board in such a manner that when the glass plate was dropped into the recess it occupied a position with respect to the pivot precisely like that occupied by it during an experiment with respect to the knife-edges of the pendulum. A V-shaped groove was cut in the upper surface of the brass rod, over a length rather greater than the breadth of the glass plate. A carefully made triangular plate of brass was furnished with two studs on its lower face, and also with an adjustable sharp scribing point, just as far perpendicularly from the line joining the centre of the two studs as the prolongation of the centre line of the V-groove was from the centre of the axle. Consequently, if the brass plate is caused to slide by means of its studs up and down the groove, the scribing point marks a line which if produced will pass through the centre of the axle. The

accuracy of this depends, of course, on the accuracy with which the groove is cut, and the accuracy of the equality of the distances between the scriber and the groove and the prolonged axis of the groove and the centre of the axle. Great care was taken to secure these conditions both in the making of the instrument and in the adjusting of the scribing point. The precise estimation in fork-beats of the distance between the breaks in the scriber lines became an easy matter with this table.

The plate was dropped into the recess, and by means of the sliding scriber two lines were drawn through the fork-line, each corresponding to radii of the pendulum and each passing through its appropriate epoch mark on one of the scriber lines. The estimation of the exact position of the epoch mark was not easy at first, owing to the slow motion of the scribers. Finally, however, by making the battery very strong, the springs also strong, the magnets very powerful, and the moment of inertia of the scribers very small, this difficulty was partially overcome. Supposing that the two lines happened to cut the fork-line in exact troughs or crests, nothing more remained than to count the fork-beats. In general, however, this was not the case, and then the following procedure had to be adopted. A sliding-table to carry a measuring microscope was arranged over the plate, and was high enough to clear the radius bar and scriber. The scriber was used to draw four lines in addition to the two marked on the plate already; the lines so drawn passed through the nearest crests or troughs of the fork-line, between which the line of section lay. A measuring microscope, with a micrometer screw of fifty to the inch and the head divided into a hundred parts, was placed on the table and adjusted by a special square, so as to be tangential to the arc of mean position of the fork at the point where that arc is cut by the epoch line. Sometimes the arc was drawn by the radius bar, the correctness of the setting of the measuring microscope was tested by observing whether the arc left the cross-wires appreciably as the microscope was screwed along. If all was right, three readings were taken with the micrometer, thus—

First reading,	cross-wires on line through	crest to right	= O.
Second	„	„ epoch	= P.
Third	„	„ crest to left	= Q.

Supposing the whole number of fork-beats has been so counted that an addition has to be made at each end to get the true length, we have: if T be the whole time required, τ the fork period, n the whole number of beats, A and A′ fractions of beats,

$$T = n\tau + (A + A')\tau.$$

Let O, P, Q be the micrometer readings as above, then—

$$A = \frac{P-O}{Q-O},$$

and similarly
$$A' = \frac{P'-O'}{Q'-O'},$$

if the rate of the pendulum be supposed constant during one fork-beat; in practice this is very nearly attained, for the firing-key is so set as to use only the middle or maximum velocity part of the stroke for scribing. In some earlier measurements the lengths between successive fork-crests were measured, with the view of applying the rigorous correction, but it was found that it differed from the one here given by less than the breadth of a fine scriber line on the smoked glass. It would, moreover, be as likely to be in excess as in defect, according as the fuse was more or less sensitive, and the firing battery stronger or weaker. Hence the whole time to be measured comes to—

$$T = n\tau + \frac{P-O}{Q-O}\tau + \frac{P'-O'}{Q'-O'}\tau.$$

In order to eliminate personal peculiarity of observation, the plates were sometimes read by one of us and sometimes by Mr. Pollock, to whom our best thanks are due. The measuring microscope was by the Cambridge Scientific Instrument Co,, and had a respectable screw; this screw was compared with a standard scale, but found to require no correction for its present purpose.

It has been stated that observations were always made alternately; first the shot was fired so that it passed the piles, say, from pile X to pile Y, and then immediately afterwards another similar charge was fired, so that the shock passed from Y to X. It is clear on general principles that if the time-constants of the gauges and scribers remain the same from shot to shot, that then, by taking the arithmetical mean of the times obtained from the plates, we shall get the time-value independent of time-constants. In order, however, to obtain additional security, both the gauges, magnets, scribers, &c., were made as much alike as possible, so as to reduce the *difference* of their time-constants to the least possible amount. Our method of elimination, however, is faulty, if there is any part of the time-constant of the gauges which is reversed by the reversal of the shot. If, for instance, the time-constants of the gauges are functions of the violence of the shock the gauges receive, then there will be outstanding errors not eliminated by taking means. In order to test this point thoroughly, a large number of experiments were made during the end of 1886 and beginning of 1887.

There may also be small differences of velocity, depending on direction caused by the configuration of the shore or sea-bottom. Finally,

and in individual readings, this was often a tiresome matter; the symmetry of the fork-line was sometimes disturbed by a superimposed small ripple of different period to that of the fork. This ripple was due to the independent vibration of the style attached to the fork. Of course, as it was small it had no effect on the whole number of beats, but made the estimation of the fractions more difficult and less accurate than would otherwise have been the case. It is not very difficult in practice, however, to estimate from a slightly rippled wave the position of the true crest, and this was always our object in making the measurements, as we hoped to be as likely to make them too large as too small, and hence to get rid of them in a number of observations. This uncertainty is, of course, entirely of the second order, and, as a matter of fact, is negligible compared with the irregularity observed, even if it is not—as we consider—entirely eliminated through the number of the observations.

Experiments to find whether the Time-constant of the Gauges depended on the Energy transmitted.

It is obviously impossible to reproduce the exact conditions of explosive pressure in a laboratory, and hence the gauges were tested with respect to their time of action by dealing blows of different intensity to the india-rubber diaphragms. This was accomplished by means of pendulums of constant lengths but of varying masses. The arcs through which the pendulums moved were also varied within wide limits. The method of testing was as follows:—The myograph was so arranged that the firing key completed the circuit of the electromagnet of a relay apparatus. The armature of the relay in its position of rest completed the circuit of an electromagnet which held the striking pendulum (by means of an iron armature) in a fixed position. The result of the fall of the myograph pendulum was, therefore, to release the pendulum suspended opposite the diaphragm of the gauge. The first scriber was arranged in such a manner that it was brought into action directly the bob of the pendulum touched the rubber diaphragm. To this end, the diaphragm was temporarily coated with a strip of tinfoil, so that the circuit of the first scriber comprised battery, tinfoil, and testing pendulum. The second scriber was in circuit with the contact portion of the gauge in the usual manner. Consequently, on dropping the myograph pendulum, a record of the usual character is obtained on the smoked plate; the first epoch is that of contact of the testing pendulum and the tinfoil, the second epoch is that of the completion of the registering contact. The pendulums employed were of different kinds. One set of experiments were made with a pendulum composed of a light brass rod, on which bobs of different sizes and materials could be screwed. In

order to eliminate any effect depending on the *velocity* of the impact of the pendulum on the rubber, this arrangement was replaced by another, in which the pendulum consisted of bobs supported by fine wire, the armatures being attached to the bobs. The bobs themselves were two in number, one being of lead and the other of plaster of Paris coated with tinfoil; the effective length of the pendulums was in each case 92 cm., the weight of the plaster arrangement complete was 160 grams, and of the lead arrangement 966 grams. The arc through which both pendulums fell was the same, and their periods were approximately equal. The result of about ten experiments with this apparatus was that the time-constant with lead bob was 0·0095 sec., and with plaster bob 0·01008 sec., showing an apparently smaller constant for the heavier blow. The energy of the blow was, by the arrangement of the apparatus, simply proportional to the masses; thus the ratio was about as 966 to 160, or about six to one, and the difference in the time-constants was 0·00058 sec. One wave of the tuning-fork, however, was rather more than a centimetre in length, and corresponded sufficiently nearly for the purpose to 0·01 sec., and the difference of the time-constants is therefore about sixty-thousandths of this, say, less than six-tenths of a millimetre on the smoked glass. This is much too large a quantity to be accounted for by experimental error in measuring the plates, but is not too large to be accounted for in other ways. It has already been mentioned that if metallic surfaces are greasy, the definition of the epoch of contact is chiefly a matter of pressure between the surfaces. Now the contact with the lead bob of the pendulum was, as far as the lead goes, on a clean surface, because the lead was scraped, but with the plaster bob the contact was between two tinfoil surfaces. Now it was found in the experiments above referred to that a much greater pressure has to be exerted to produce "contact" when the surfaces are greasy than when they are clean, and tinfoil is always greasy, and consequently we should expect that, as far as this effect goes, the time-constant would appear *longer* with the lead bob than with the plaster one. This is contrary to what was observed. On the other hand, the tinfoil on the plaster was slightly creased, and hence, if we suppose that a sensible wave of compression does not begin to travel up the tube till the deformation of the india-rubber has progressed to a certain extent, the effect of the tinfoil creases will be to exaggerate the difference between the time-constants in the same sense as that observed. We conclude, therefore, that, setting aside the difference in the rate of propagation of the waves in the gauge tube depending on their amplitude, we must look to the moving part of the contact apparatus for an explanation. This was considerably heavier than was ultimately used in the actual experiments, and the adjustment was nothing like so delicate as we after-

wards succeeded in making it, and consequently there is every reason to believe that the difference in time-constant observed in these experiments was very much greater than it was in the actual observations. But the times measured are of the order of one-tenth of a second, and the amount we are now concerned with is of the order of 0·0006 second, so it is clear we are discussing the correction of a correction.

It is true, however, that the instantaneous pressures to which the gauges are exposed during explosions differ by considerably greater amounts than those due to the action of the two pendulums; it was always arranged, therefore, that the gauges should have a considerable excess of sensitiveness when used to measure the explosion velocities. It must be added that the length of the chord of the arc through which the pendulums swung in the testing experiments was about 20 cm., so that the whole time in question is only of the order of the time taken by the pendulums to swing through about half a millimetre at their greatest velocity. In addition to this, it was extremely difficult to keep the tinfoil properly and uniformly glued to the surface of the rubber. The effect of the blow was always to crumple the tinfoil to a small extent; a wrinkle of the order of 0·1 mm. would explain the whole effect, and after several impacts much greater deviations were difficult to avoid. The blows from the lead bob resulting in by far the greatest deformation of the rubber produced much the greatest trouble in wrinkling the tinfoil, and yet we could not replace the tinfoil after such experiment because we desired to be sure that the state of the surface as to greasiness was constant, and consequently we were obliged to smooth the tinfoil down to the best of our ability. It may be added that, of course, the tinfoil was as well cleaned as possible, but it is not a very easy substance to be quite sure about in this respect, and the thinnest silver and platinum foil we had and tried would not stick to the rubber properly unless stuck by so much marine glue as to weight the diaphragms sensibly, and thus perhaps introduce a peculiarity. Having in view the sources of error above indicated, we did not feel justified in applying any correction to the observed velocities of explosion-effect on this account; the effect, therefore, will be that our velocities *run the risk* of being a little too high to the extreme extent of, say, 2 per cent.; we are certain ourselves, however, that this is not the case. It may be mentioned that the experiments with the rod pendulums led, on the whole, to rather smaller values for the time-constants, but we have not laid any stress on them, as the pendulums did not move with exactly equal velocities in all cases.

Comparison of the Fork with a Clock.

The fork employed was a very excellent and massive one, belonging to a ribbon chronograph apparatus and made by Koenig. The fork was marked 100 VD. We felt confidence in this fork from the first, as it was *supplied* with a style and platinum cord contact apparatus for electric maintenance. A comparison was made with the laboratory clock (a very fine one, by Cooke, of York) by one of us and Mr. Arthur Pollock. Lord Rayleigh's method was employed. The fork compared directly with the clock was also by Koenig, and was reduced from about 35 complete vibrations per second to 33·194 approximately, by very carefully equalised weights. The approximate period was found by counting whole and estimating partial vibrations, over a period of 27 seconds; the chronograph above alluded to was used for this purpose—one scriber marked periods of 2 seconds direct from the clock, and the other was included in the driving circuit of the fork. The length of ribbon used was 56 feet. We insist on the accuracy of this approximate measurement, because the exact comparison requires it. The clock, with its contact arrangement, had been carefully rated for some time and adjusted so that for this purpose the rate is negligible—it was about a second a day. The observation of the light spot was undertaken by Mr. Pollock; it was found most convenient to interpose the adjustable slit of a spectroscope between the object-glass and the eye-piece of the observing telescope. The pendulum of the clock made a complete vibration in 2 seconds, and the slits of the fork opened twice in each complete vibration. The period of recurrence of the flash system was 432 seconds, and the fork was losing on the clock.

The result is that the actual frequency of the fork is 33·1933 complete vibrations per second. The second harmonic of this was the frequency of the driven fork with which the standard was compared.

Hence frequency of driven fork is—

$$99{\cdot}5799.$$

The beats were counted over a period of five minutes, the driven fork being at 20° C. by a Kew standard (and re-examined) thermometer. This temperature was then changed to 30° C., and the beats again counted. Two sets of experiments were made, which agreed very well together. During the observations at 30° C. the driving-fork period of flash recurrence changed to about 270 seconds. The final result, the calculation of which presents no new features, is that

$$n = 100{\cdot}2622 - 0{\cdot}01472\, t,$$

n being frequency at t° C., on the assumption that the temperature coefficient is constant. In practice, the lowest temperature on any

day was 13° C. This result amounts to making the fork correct at 17·81° C. An experiment was made to try whether the slight damping of the fork, produced by the friction of the style on smoked glass, caused any appreciable change in the period. A plate of smoked glass was set up so that the style rubbed the glass, and the beats were again counted. If any effect exists it is less than an alteration of frequency corresponding to one complete vibration in two minutes.

The effect on the observed velocity of transmission of explosive energy at the extreme temperatures of 13·4° and 28° of the temperature coefficient of the fork is 0·065 and 0·15 per cent. respectively, of the uncorrected value, and is, of course, included in the reduction of the observations.

Measurement of Distance between the Gauges.

For the purpose of estimating the distance between the piles a spike was driven into the top of each, to serve as a mark for the cross-wires of the theodolite. No less than three distinct sets of measurements were made for us by Mr. R. J. A. Roberts, Government Surveyor; this was due to the fact that on one occasion one of the piles was pulled up by a large steamer, which made fast to it in order to swing, and after the pile had been re-driven rather badly it got washed to one side by the heavy wash of another steamer. Finally the pile was moved further away, a better bottom was found, and we had no further trouble. In the intermediate position the pile was not driven quite straight, consequently the distance between the gauges had to be corrected according to their depth. Now the gauges were made to follow the tide, and consequently the correction had to be applied to each experiment. For this purpose, some measurements of the slope were made and a table of corrections for what we called immersion was prepared, by whose aid the distance on each occasion was correctly arrived at. The gauges were fastened to the piles in such a way that no appreciable correction has to be applied to Mr. Roberts's distance between the spikes. We do not mean to say that there may not be some small difference arising from the thickness of the drum (about 3 cm.) and accidental straining of the woodwork, but it cannot amount to more than a few centimetres, and will vary from day to day, as probably in one direction as in another. The piles were also very possibly a little bent by the wind and tide; the amount of this could not be estimated without keeping a theodolite permanently set up, and this we could not do, partly because we had no theodolite of our own. However, this small error is as likely to be in excess as in defect.

Much more important for the absolute velocity is the displacement of the firing buoy, by wind and tide, from the direct line of the piles.

The buoys were anchored with sinkers and attached to the pile by ropes, so as to keep them as rigidly as possible in one position. However, at low tides the ropes became slack, and then if a wind was blowing, the shot drifted for a few yards out of the direct line; the result will be that the second gauge may be affected by a portion of the wave front (if there is one) which is not identical with the portion affecting the first. Owing, however, to the end of the mooring line being permanently fastened to the nearest pile, the shot when displaced will practically move in an arc of a circle, with the pile as centre, and hence we have a means of estimating the maximum effect of any amount of drifting. It can readily be seen by drawing the figure that drifting of the shot will have the same effect as diminishing the distance between the piles, and hence the times will be rather shorter, and the velocities rather greater, than would otherwise be the case. An error of this kind will not, therefore, be eliminated by any number of observations, as it is always in one direction. The remedy is to take as much pains as possible to prevent its occurrence, and this was always done; an account of the magnitude of the error which may arise in this way is contained in the third part of this paper.

Method Adopted to Determine the Distance between two Spikes in Piles driven out in the Water, Berry's Bay, Port Jackson.

The base-line was measured with a 100 link $\frac{1}{4}$-inch steel riband with adjustable ends, to which was applied a tension equal to 16 lb. The chain was found after careful testing to be equal to the standard at the Government Observatory, Sydney, at a temperature of 78° F. It was tested on a calm, cloudy morning, the temperature being even, and the base-line was measured the same day, the temperature being at the time 61° F. The standard at the Observatory is $\frac{4}{100}$ths of an inch short, it having been measured off with the compensating rods for measuring trig.-base lines. A correction was therefore applied to make up for this error. The chain when tested was supported throughout its entire length, but was suspended when used for measuring the base-line. By careful experiment it was found that this made a difference in length of $\frac{8}{100}$ths of an inch, for which a correction was also applied. The expansion or contraction being equal to 0·005 of 1 inch for 1° F. for one chain, the distance in chains, multiplied by half the difference between standard and observed temperature, and the result divided by 8, gives a correction in decimals of a link to be added or subtracted, as the temperature is higher or lower than the standard temperature.

To the handle at the back end of the chain was attached a small

piece of steel with a knife-edge, and this was regarded as the end of the chain. This end was worked by means of a slow motion screw attached to a board, on which the chainman knelt, and so the end of the chain was kept perfectly steady. The knife-edge was brought by the screw to the mark, which was a very fine pencil dot on a piece of white paper, gummed on to a flat-headed nail driven into the ground. On the forward end of the chain was hooked a spring balance, which indicated a 16 lb. strain, applied by means of a light straining pole, 12 feet long, thus enabling the chainman to apply the strain far more steadily and easily than by any other method at present in use; and also allowing the full length of the chain to be used on a slope equal to about 1 in 5, the end of the chain sliding up and down the pole. The greatest slope in this case was, however, only about 1 in 10, and only for two chains of the entire length. The forward end of the chain was taken as the position of a small hole, through which a string, with a small plummet attached, passed. This plummet was used merely to indicate the position of the nail, and to keep in adjustment a contrivance which gave the position of the transit theodolite (carefully adjusted for the telescopic axis) at right angles to the direction of the chain. The end of the chain was sighted by means of the transit instrument, and the telescope being then depressed, the fine point of a hard pencil was lined on the paper before described. This method does away with any errors due to the use of the plummet (especially in windy weather) for marking the end of the chain on sloping ground. By the use of the instrument the end of the chain can be marked to $\frac{1}{100}$th of an inch. This method has also other advantages for accurate measurement.

The instrument used was a 5-inch transit theodolite, by Troughton and Simms, of London, having the circle graduated to 20 seconds. The telescope was larger than, and of much superior quality to, those usually fitted to 5-inch instruments. The plummet was suspended from the end of the vertical axis. The instrument was used on a traversing top stand, and being in very good order, and the axis moving freely, there was no twisting pressure (torsion) on the stand, and the day being a cloudy one, and the sun completely obscured, the effect of sunlight on the eyes was avoided. The instrument was carefully set up over each of the points A, B, C, and D on the base-line, and angles were observed to the spikes in the piles (points E and F) off line AD. The angles shown on the plan are the means of four readings at each of the stations. After the first two readings the telescope was turned over in the reversed position, so that any possible errors due to change of focus (movement of the optical axis of the instrument), want of horizontality of the telescopic axis, and imperfect collimation, are entirely eliminated. The levels were sensitive, and of good quality. The readings of both verniers were recorded

and a mean taken, and each angle was read off a different part of the graduated circle each time, so that any error due to eccentricity was entirely eliminated, and any errors due to imperfect graduation were reduced to a minimum. In repeating an angle on the graduated circle, not only is the second measures of the angle given on a different part of the circle, but an angular quantity, which perhaps the vernier is incapable of showing, has been doubled, and may therefore become apparent.

As an example, suppose the instrument carefully set over the point A (see Plate 3) and levelled, and observing to D as an initial point, the two verniers are read off, and their position recorded in the field-book. Leaving the lower clamp fast, and releasing the milled-headed screw clamping the vernier plate and that carrying the graduated circle, the point E is then observed to, and the readings recorded, and so on to the point F. Then leaving the upper clamp fast and releasing the lower, the point D is again referred to, and the operation repeated *three* times, thus getting four measures for the angles, taking care after the second round to reverse the telescope. This movement will bring the vernier originally on the left hand side of the instrument to the right hand. A mean of the verniers and all the readings is the required angle. Tripods with fine steel points and heavy plummets were used as reference marks carefully set over the ends of the base-line.

The alignment of the points B and C was very carefully performed, using the telescope in reversed positions, both from the ends of the line and at the points themselves.

The triangles obtained by the above operations for fixing E and F were too small to make it necessary to go into the equation of their conditions.

Part II.—Reductions of Observations and Calculation of the Theoretical Velocity of Sound through Water.

Distance over which Disturbance Travelled.

The distance between the gauges was at first 15,410 cm., it was afterwards 15,840 cm., minus a correction made for the slope of the pile which had been driven slightly out of the vertical. This correction varied between 51 cm. and 59 cm., and depended on the depth of the gauge below the top of the pile. Finally in the third position the distance was 18,210 cm.

Time Measurements.

Let t be the epoch of arrival of the disturbance at gauge A, and let τ_1, σ_1 be the time constant of the gauge and of the scriber attached to it respectively, then the record on the smoked plate occurs at

time $t+\tau_1+\sigma_1$. Similarly if T be the time required for the transmission of the disturbance, then the second record on the smoked plate occurs at time $T+t+\tau_2+\sigma_2$, where τ_2, σ_2 are the time-constants of the second gauge and its attached scriber respectively.

Thus the observed interval as recorded on the smoked plate is—

$$\tau = T+\tau_2+\sigma_2-\tau_1-\sigma_1.$$

Similarly for a disturbance in the opposite direction the recorded interval is—

$$\tau' = T+\tau_1+\sigma_1-\tau_2-\sigma_2,$$

provided that the time of transmission is the same in both directions, and that the time constants of the gauges and scribers have not altered between the two observations, and that the scribers are attached to the same gauges in the two observations.

Hence time of transmission of disturbance $= \frac{\tau+\tau'}{2}$.

The velocity of transmission calculated on the supposition that the fork has an exact frequency of 100 must be multiplied by $1\cdot002622-0\cdot0001472\,t$, to get the correct velocity.

The velocities had been originally deduced on the supposition that the fork was correct; these uncorrected velocities are given in Column 9 of the Table; the correction arising from the decimal parts of the above factor, viz., from $0\cdot002622-0\cdot0001472\,t$, are given in Column 10, and the corrected velocities in Column 11.

The complete formula for V the velocity is thus—

$$V = \frac{S}{\left(\frac{\tau+\tau'}{2}\right)}\{1\cdot002622-0\cdot0001472\,t\},$$

where S is the distance between the gauges and t is the temperature of the fork. This gives the velocity at the temperature of the sea-water during the observation.

Theoretical Calculation of the Velocity.

In Column 12 of the Table is given the velocity of sound, calculated theoretically as follows. The formula giving the velocity of sound through water is taken to be—

$$V = \sqrt{\frac{E_\phi}{D}},$$

where E_ϕ is the adiabatic resilience of volume, and D is the density at the temperature considered.

Observation and Calculation of the Density.

In the "Challenger Report," vol. 1, 'Physics and Chemistry,' p. 70 *et seq.*, is given a table for calculating the density of a sea-water at any temperature when its density at 15·56° C. is known. To get the density of the Berry's Bay water at this temperature six experiments were made on two samples collected at an interval of a month. The water of the Bay is practically Pacific Ocean water, there being to all intents and purposes no fresh water flowing into the Bay. Four of the experiments were made by weighing a piece of glass (English flint), both in doubly vacuum-distilled water and in the sea-water; and two experiments were made with a specific gravity bottle. Both the glass and the water had been allowed to stand in the balance case for seven hours before weighing; the thermometer used was a standard compared at Kew, the correction being, as it happened, zero, for the temperature observed. The piece of glass was suspended from the arm of the balance by a platinum wire, the length of which was 28 cm. The weight of 1 metre of the wire was 0·1004 gram, and the density was taken as 21. In each weighing in water one half the wire was immersed. The sensitiveness of the balance was such as to give readings of eight scale divisions per milligram. The density of the sea-water is compared with that of pure water by means of the formula

$$\rho\left\{\frac{G}{g}+\frac{\frac{1}{2}P}{p}\right\}=(W-W')\left\{1-\frac{\sigma}{B}+\frac{\sigma}{\rho}\right\},$$

in which ρ is the density of the water in which the glass is weighed,

G = mass of glass,
g = density of glass,
W = apparent weight in air,
W' = apparent weight in water,
σ = density of the air during weighing,
B = density of the brass weights = 8·4,
P = mass of platinum wire = 0·028112 gram,
p = density of platinum = 21.

ρ on the right-hand side is taken approximately.

The above formula is got by neglecting

$$-(W-W')\frac{\sigma^2}{\rho B}+\left(\frac{G}{g}+\frac{\frac{1}{2}P}{p}\right)\frac{\sigma^2}{\rho},$$

compared with $W-W'$.

In the present case this is equivalent to neglecting—

0·00000815 gram in comparison with 4·93 grams.

By applying the above formula to the weighing in sea-water at 17° C. and to the weighing in pure water at 16·65° C., the first experiment gave density of sea-water at 17° = 1·02702 density of pure water at 16·65°. The density of pure water at the various temperatures was taken from Kupffer's observations as reduced by Professor W. H. Miller (Everett's 'Units and Physical Constants,' 2nd edition, p. 39). The densities for temperatures intermediate to those given by Everett were obtained by interpolation.

For the measurement of the density by means of the specific gravity bottle, two bottles were used, one acting as a counterpoise, together with the small requisite quantity of sand and aluminium foil. The other bottle being quite clean was then filled with the water to be weighed, great care being taken to avoid the retention of any bubble of air. The dry and wet bulb thermometer apparatus was observed, together with the barometer, in order to make the usual corrections for weighing in air. The results of the two measurements made by this method agreed closely with the other four measurements made by weighing glass. In order to have comparable results and to get a mean, the results of the measurements made at 17°, 18·6°, 18·65°, 19·8°, and 21° C. respectively were reduced by "Challenger Report," 'Physics and Chemistry,' vol. 1, p. 70, to the temperature 15·56° C. These comparable results are given in the following table.

Weights were assigned to the observations as shown.

	Experiment.	Density at 15·56° C.	Mean.	Weight assigned to observation.
By weighing glass ..	1	1·02623		
" ..	2	1·02629203	1·026271	3
" ..	3	1·02629205		
" ..	4	1·02599	1·02599	4*
By sp. gr. bottle ..	5	1·02628	1·026155	1
" ..	6	1·02603		

Hence the probable value of the density at 15·56° C. is 1·026116 grams per cubic centimetre.

The densities of the water at the various temperatures of the experiments at the Bay were thence found by means of the table in the "Challenger Report."

The temperatures of the water in the Bay during the experiments were—

* The high weights assigned to this observation are because the weighings were taken at midnight.

13°, 13·3°, 13·4°, 13·5°, 14·1°, 14·3°, 14·5°, 15·2°, 15·3°, 15·5°, 15·8°, 16°, 17°, 17·5°, 17·6°, 18°, 18·5°, 20° C.

The densities are calculated for these temperatures, and also for 0·1° below and above each.

This was done in order to find the coefficient of expansion of the sea-water, since this occurs in getting the adiabatic resilience from the isothermal resilience. The result is as follows:—

Temperature.	Density.	Temperature.	Density.
12·9°	1·026676	15·7°	1·026086
13·0	1·0266567	15·8	1·026056
13·1	1·026636	15·9	1·026036
13·2	1·026616	16·0	1·026016
13·3	1·026596	16·1	1·025996
13·4	1·026576	16·9	1·025806
13·5	1·026556	17·0	1·0257856
13·6	1·026536	17·1	1·0257656
14·0	1·026456	17·4	1·0256855
14·1	1·026436	17·5	1·0256655
14·2	1·026416	17·6	1·0256355
14·3	1·026386	17·7	1·0256154
14·4	1·026366	17·9	1·0255554
14·5	1·026346	18·0	1·0255353
14·6	1·026326	18·1	1·0255053
15·1	1·026216	18·4	1·0254352
15·2	1·026196	18·5	1·0254052
15·3	1·026176	18·6	1·0253852
15·4	1·026156	19·9	1·0250449
15·5	1·026126	20·0	1·0250149
15·6	1·026106	20·1	1·0249949

These values give the mean coefficient of expansion 0·000221 per degree of temperature between 12·9° and 20·1° C.

Determination of the Resilience.

In accordance with the laws of thermodynamics, the adiabatic resilience E_ϕ is taken to be $\frac{c_p}{c_v}E_\theta$, where c_p, c_v are the specific heats at constant pressure and constant volume respectively, and E_θ is the isothermal resilience.

Determination of the Ratio of the Specific Heats.

The ratio $c_p \div c_v$ is found as in Clausius ('Translation,' p. 181), where it is shown that

$$c_v = c_p + \frac{T}{J} \cdot \frac{\left(\frac{d_p v}{dT}\right)^2}{\frac{d_T v}{dp}}.$$

Here T is the absolute temperature and J is Joule's equivalent. The equation may be written—

$$\frac{c_v}{c_p} = 1 + \frac{T}{Jc_p \cdot \frac{1}{v}} \cdot \frac{\left(\frac{1}{v}\frac{d_p v}{dT}\right)^2}{\frac{1}{v}\frac{d_T v}{dp}},$$

in which $\frac{1}{v}\frac{d_p v}{dT}$ is the coefficient of expansion, and $\frac{1}{v} \cdot \frac{d_T v}{dp}$ with a negative sign is the isothermal compressibility, or the reciprocal of the isothermal resilience.

Thus $\frac{c_p}{c_v} = \frac{1}{1 - \frac{TE_\theta e^2}{Jc_p . D}}$, if e denote the coefficient of expansion, and D the density which is put for $\frac{1}{v}$.

Thus it is requisite to know c_p, the specific heat, besides the quantities represented by the other letters.

With a view to obtaining c_p, it was discovered from Watts's 'Dictionary of Chemistry,' vol. 5, p. 1017, that the composition of Pacific Ocean water at a depth of 11 feet, Lat. 25° 11′ S., Long. 93° 24′ W., is—

Na	10261·9	parts in 1,000,000.
Cl	18949·7	,,
SO_4	2786·4	,,
Mg	1315·1	,,
K	603·8	,,
Ca	471·9	,,
Br	310·2	,,

making the total solid content 3·47 per cent. approximately.

Again, in vol. 7, 2nd Supplement, p. 598, it is stated that a solution $NaCl + 100H_2O$ has a specific heat 0·962, with a specific gravity 1·0234; and this solution contains approximately 3·14 per cent. solid matter. Thus, since c_p occurs in a small term, it may be taken without much error = 0·962. An experiment made by Mr. Flint, one of the students in the Laboratory, gave a value practically the same as the value here taken.

The value of e has been previously found to be 0·000221. J is taken 42×10^6, and D the density given in the Table.

Determination of the Isothermal Resiliences.

With regard to E_θ, Professor Tait has kindly supplied us with information. He gives formulæ for the compressibility of fresh water and of sea-water at low pressures and high pressures. The compressibility, as given by him, is a function of the pressure and the temperature, and thus for a pressure which varies rapidly from a low value to such a great value as one or three tons weight per square inch, the compressibility would be a variable quantity. It did not appear at first whether his formula for moderately low pressures, or the formula for such pressures as from one to three tons per square inch, was to be used to get E_θ, the reciprocal of the compressibility. The further uncertainty as to the effect of viscosity is not allowed for in finding the theoretical velocity. With a view to settle which of Tait's formulæ was to be adopted, it was remarked that for an incompressible uniform liquid, subject to impulsive pressures, the equation to determine the impulsive pressure at any point is (Lamb, p. 12)—

$$\frac{d^2\varpi}{dx^2}+\frac{d^2\varpi}{dy^2}+\frac{d^2\varpi}{dz^2}=0.$$

And hence, for an impulsive pressure uniformly distributed over a sphere, the impulsive pressure at a point outside the sphere would be inversely proportional to the distance at that point from the centre of sphere.

It seems desirable to determine the function ϖ of x, y, z, and t, which will satisfy given boundary conditions and the equation

$$\frac{d\rho}{dt}+\frac{d^2\varpi}{dx^2}+\frac{d^2\varpi}{dy^2}+\frac{d^2\varpi}{dz^2}=0,$$

arising from the equation of continuity combined with the equations of impulses, when a relation is assumed between ρ and ϖ. If it is supposed that impulsive pressure is subject to the laws of ordinary pressure, such a relation as that given by Van der Waals might perhaps be taken, or one of Tait's relations giving the compressibility as a function of pressure and temperature.

Supposing that the above consideration approximately applies to sea-water, it was further concluded, by the method of Berthelot in discussing the experiments of Sarrau and Vieille, that in our case the initial pressure on the walls of the case containing the explosive was in each case (guncotton and dynamite) about 8000 kilograms weight per sq. cm. This agreement between the initial pressure due to guncotton and that due to dynamite is accounted for by the density of charge being rather different in the two cases. Supposing this pressure uniformly distributed over a sphere of 1 foot diameter,

which is very much greater than the volume actually occupied by the explosive, and that the pressure at a distant point is inversely proportional to the distance from the centre of the sphere, the pressure at 20 yards distant would be approximately 64·4 atmospheres of 1,014,412 dynes per sq. cm. each. This value for an atmosphere has been taken as representing Tait's value, which he defines by 152·3 atmospheres = 1 ton weight per square inch, at Edinburgh (?). For pressures considerably below this, Tait's low pressure formula is applicable. If the sphere supposed to be occupied by the explosive is less than 1 foot in diameter, the pressure at a distant point would, on the above suppositions, be proportionately diminished; and at greater distance than 20 yards it would be further proportionately diminished. Viscosity would aid in further diminishing the pressure, so that on the whole, for the space between the gauges, it seemed advisable to use the low pressure formula as given by Tait.

This formula is—

Average compressibility of sea-water at low pressures is

$$481 \times 10^{-7} - 340 \times 10^{-9}\,t + 3 \times 10^{-9}\,t^2$$

per atmosphere increase of pressure at temperature t, 152·3 atmospheres being = 1 ton weight per square inch.

Taking such an atmosphere as 1,014,412 dynes per sq. cm., we thus get—

$$E_\theta = \frac{1014412}{481 \times 10^{-7} - 340 \times 10^{-9}\,t + 3 \times 10^{-9}\,t^2}.$$

The following table gives the values of this for the temperatures of the observations.

Temperature.	Coefficient of resilience, E_θ.	Temperature.	Coefficient of resilience, E_θ.
13·0	$2{\cdot}2957 \times 10^{10}$	15·5	$2{\cdot}3293 \times 10^{10}$
13·3	2·2998	15·8	2·3332
13·4	2·3011	16·0	2·3358
13·5	2·3025	17·0	2·3489
14·1	2·3106	17·5	2·3553
14·3	2·3133	17·6	2·3566
14·5	3·3160	18·0	2·3617
15·2	2·3253	18·5	2·3681
15·3	2·3266	20·0	2·3868

General Formula for the Velocity of Sound.

The formula on p. 516 thus becomes—

$$\text{Velocity} = \sqrt{\frac{1}{\frac{D}{E_\theta} - \frac{T e^2}{J c_p}}};$$

or, substituting the values already found for sea-water,

$$\text{Velocity} = \sqrt{\frac{1}{\frac{D(481\times10^{-7}-340\times10^{-9}t+3\times10^{-9}t^2)}{1014412}-\frac{(273+t)(\cdot000221)^2}{42\times10^6\times\cdot962}}},$$

As stated before, the velocities thus calculated are given in column 12 of the general table. The calculation is laborious.

Explanation of the General Table.

The Table explains itself except with regard to the observations on July 11, and the three first on August 10. In these cases a mean of three intervals was taken by taking a mean of two shots in the direction left to right, and then a mean between that mean and the interval from right to left. It is to be remarked that in the case of the pair of shots on September 13, which gave the mean interval 0·1277, the mark on the smoked plate determining the single interval 0·1210 was extremely faint. A photograph of this plate is shown (p. 524), the single radial line showing the slight break in the line traced by the scriber.

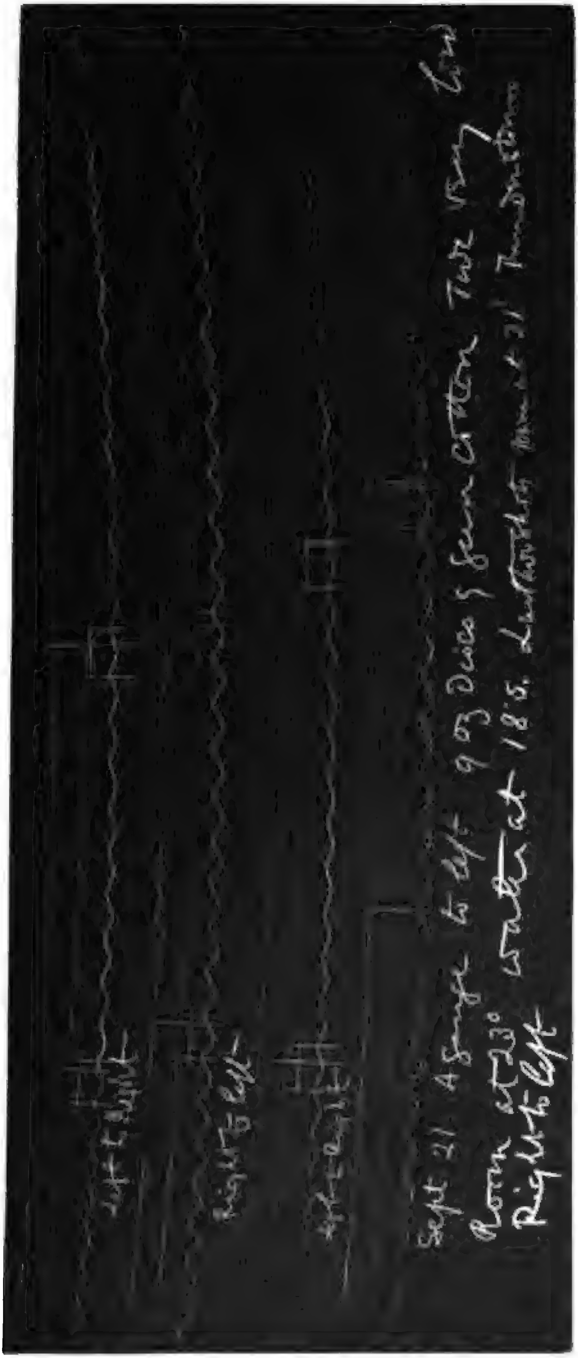
On the lower part of the General Table are given the observations from which a mean interval could not be obtained, together with the first observations on April 25 and May 7, and another pair of observations on July 5. These observations were rejected on account of the gauge having been water-logged; besides this they should be regarded with suspicion, since they would give results whose departure from the final mean would be greater than the "maximum error" found in accordance with the theory of adjustment of observations.

It may be added that as a general result of our experience we found that (possibly owing to some interference effects) the distant gauge was often more violently affected after its first indication. Now when the gauges were water-logged they became deficient in sensitiveness, and consequently in the observations referred to the probability is that the mark observed corresponded to the *second*, not the *first* shock. This conclusion is strengthened by a reference to the photograph of the plate of September 13.

PART III.—REDUCTION AND DISCUSSION OF RESULTS.

The experiments are divided into four classes, according to the nature and quantity of the explosive used. In Class A the explosive was a 9-oz. disk of guncotton, in Class B 10 oz. of dynamite, in Class C 18 oz. of guncotton, and in Class D, 4 lb. of guncotton. In


Right to left
Left to Right
Right to left


Left to right
Right to left
Left to right
Sept 21 A Single to left 9 oz Disc ¼ from centre Tube very
Room at 23° water at 18°5
Right to left

Class A the velocity seems to increase with the temperature, but no such law was detected in the other classes.

In order to reduce the observed velocities (when corrected for the temperature of the fork) to a comparable state, they are reduced to one temperature; and in the absence of a rule for doing this it is supposed that each velocity in Class A, &c., is expressible in the form $V_A + at$, where V_A denotes the velocity at some temperature between the extreme temperatures of the observations in Class A, and t is the excess of the temperature of the observation above the chosen temperature. For Class A the temperature chosen was the mean of the temperatures of the observations; this mean is approximately 17·791° C. Hence arises the system of equations

$$
\begin{aligned}
V_A-0{\cdot}191a &= 167567\\
V_A-0{\cdot}191a &= 165943\\
V_A-0{\cdot}291a &= 157308\\
V_A-0{\cdot}791a &= 157791\\
V_A-0{\cdot}791a &= 148804\\
V_A+0{\cdot}709a &= 180697\\
V_A+0{\cdot}709a &= 193839\\
V_A+0{\cdot}209a &= 197940\\
V_A+0{\cdot}209a &= 163439\\
V_A+0{\cdot}209a &= 197540\\
V_A+0{\cdot}209a &= 174601.
\end{aligned}
$$

If the method of least squares be considered applicable, the equations for the probable values of V_A and a are (Stewart and Gee, vol. 1, p. 274):

$$
\begin{cases}
11V_A-0{\cdot}001a = 1905469.\\
-(0{\cdot}091)V_A+2{\cdot}5891a = \quad 66858.
\end{cases}
$$

These give—

$$V_A = 173227, \qquad\qquad a = 25890.$$

This large value of a will be merely used in getting the probable error of the mean, *i.e.*, of the above value of V_A. If it has a physical meaning it is very noteworthy. Substituting the value 25890 for a in the foregoing equations there arise the values:—

	Departure from mean.
V_A = 172512	−715
170888	−2339
164842	−8385
178270	+5043
169283	−3944

	Departure from mean.
V_A = 162341	−10886
175483	+2256
192529	+19302
158028	−15199
192129	+18902
169190	−4037

Mean = 173227, approximately as above.

By Bessel's formula, probable error of mean

$$= 0{\cdot}6745\sqrt{\frac{\text{sum of square of departures}}{10\times 11}} = \pm 2244.$$

By Peters's formula, probable error of mean

$$= 2212.$$

Thus the result for Class A may be written—

Probable velocity at 17·79° C. = 1732±22 metres per second.

The theoretical velocity of sound is 1523 metres per second.

Treating Class B similarly, the mean temperature of the observations is 14·546° C. For convenience of calculation V_B is taken to be the velocity at 14·5°, and the system of resulting equations is:—

Temp. of water.	
14·5° C.	$V_B+0b = 156990$
14·8	$V_B+0{\cdot}3b = 170198$
13·0	$V_B-1{\cdot}5b = 149256$
13·0	$V_B-1{\cdot}5b = 150393$
13·9	$V_B-0{\cdot}6b = 193252$
13·3	$V_B-1{\cdot}2b = 154059$
13·3	$V_B-1{\cdot}2b = 154965$
13·3	$V_B-1{\cdot}2b = 152420$
13·3	$V_B-1{\cdot}2b = 155116$
13·5	$V_B-1{\cdot}0b = 175264$
13·4	$V_B-1{\cdot}1b = 191406$
13·4	$V_B-1{\cdot}1b = 188662$
15·5	$V_B+1{\cdot}0b = 214710$
15·5	$V_B+1{\cdot}0b = 195989$
15·5	$V_B+1{\cdot}0b = 212728$
15·5	$V_B+1{\cdot}0b = 198361$
15·2	$V_B+0{\cdot}7b = 201076$
15·2	$V_B+0{\cdot}7b = 180730$
15·2	$V_B+0{\cdot}7b = 199025$
15·2	$V_B+0{\cdot}7b = 179417$
16·0	$V_B+1{\cdot}5b = 186556$

Temp. of water.	
16·0	$V_B+1{\cdot}5b = 142538$
15·8	$V_B+1{\cdot}3b = 192178$
15·8	$V_B+1{\cdot}3b = 174657$

The equations for the probable values of V_B and b are—

$$\begin{cases} 24V_B + \ \ 1{\cdot}1b \ = 4269946, \\ 1{\cdot}1V_B + 27{\cdot}97b = \ \ 476912. \end{cases}$$

and

Hence $V_B = 177453$, $b = 10072$.

The smallness of b compared with a, the corresponding quantity in Class A, may perhaps be referred to the greater number of observations in Class B; yet it would not seem to represent the real increase of velocity due to an increase of 1° of temperature.

In order to get the probable error of the mean, viz., 177453, using the above value of b, the above equations become—

	Departure from mean.
$V_B =$ 156990	−20463
167176	−10277
164364	−13089
165501	−11952
199295	+21842
166145	−11308
167051	−10402
164506	−12947
167202	−10251
185336	+7883
202485	+25032
199741	+22288
204638	+27185
185917	+8464
202656	+25203
188289	+10836
194026	+16573
173680	−3773
191975	+14522
172367	−5086
171448	−6005
127437	−50023
179084	+1631
161563	−15890

Mean as before 177453

Probable error of mean (Peters's) = 2665.

Thus the result of Class B may be expressed—

Probable value of velocity at 14·5° C = 1775 ± 27 metres per second.

The theoretical velocity of sound is 1508 metres per second.

For Class C the mean temperature is 18·3° C. Using V_C to represent the velocity at 18·3°, and c being a quantity similar to the quantity a in Class A, the system of resulting equations is—

$$
\begin{aligned}
V_C - 0{\cdot}3c &= 197354 \\
V_C - 0{\cdot}3c &= 199519 \\
V_C + 0{\cdot}2c &= 192756 \\
V_C + 0{\cdot}2c &= 193659 \\
V_C + 0{\cdot}2c &= 187674
\end{aligned}
$$

$$
\text{Hence probable value of } V_C = 194192, \qquad c = -14147.
$$

Substituting this value of c above, the values are—

		Departure from mean.
V_C =	193110	−1082
	195275	+1083
	195585	+1393
	196488	+2296
	190503	−3689
Mean as above =	194192	

If the method of least squares be applicable to such a small number of values, then probable error of mean (Bessel) = 726; or by Peters's formula probable error of mean = 807. Thus the result of Class C may be written—

Probable velocity at 18·3° C. = 1942 ± 8 metres per second.

The theoretical velocity of sound is 1525 metres per second. For Class D, reducing the values to 19° C., the equations become—

$$
\left\{
\begin{aligned}
V_D &= 210000 \\
V_D &= 192568 \\
V_D + d &= 192327
\end{aligned}
\right.
$$

Hence probable value of $V_D = 201284$

" " $d = -8957$

The values of V_D are therefore—

		Departure from mean.
V_D =	210000	+8716
	192568	−8716
	201284	0
Mean as above	= 201284	

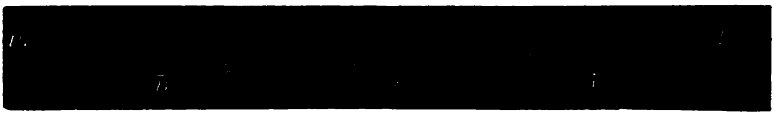
Hence probable error of mean (Peters) = 3474
„ „ „ (Bessel) = 3394

Thus the result of Class D may be written—

Probable velocity at 19° C. = 2013±35 metres per second.

The theoretical velocity of sound is 1528 metres per second. Thus in each class the experimental velocity is greater than the theoretical velocity of sound.

During the experiments it sometimes occurred that the firing buoy drifted slightly (by influence of wind and tide) from the line containing the piles. The deviation from that line could only be approximately estimated, but it was judged that it was never so much as 3·5 metres, although it was usually slightly out of line. Suppose its distance from the line of the piles to be 3·5 metres, and that the deviation at the other end was the same, then (see figure) taking the distances as marked, A and B being the piles,



P the firing point, if the explosion occurs at time t, and if V be the velocity of transmission of the disturbance, this reaches A at time $t+\frac{AP}{V}$ and reaches B at time $t+\frac{BP}{V}$, and the first interval recorded on the smoked plate is

$$\pi=\frac{BP-AP}{V}+\tau_2+\sigma_2-\tau_1-\sigma_1.$$

The second interval of the shot from P′ is

$$\pi'=\frac{AP'-BP'}{V}+\tau_1+\sigma_1-\tau_2-\sigma_3.$$

Hence, if $$BP-AP=AP'-BP',$$

$$\frac{\tau+\tau'}{2}=\frac{BP-AP}{V}.$$

and
$$V = \frac{BP-AP}{\left(\frac{\tau+\tau'}{2}\right)}.$$

But the velocity, calculated on supposition of the firing point being in line, is
$$V' = \frac{AB}{\left(\frac{\tau+\tau'}{2}\right)}$$

$$\therefore \frac{V}{V'} = \frac{BP-AP}{AB} = \frac{\sin\alpha-\sin\beta}{\sin\theta} = \frac{\cos\frac{\alpha+\beta}{2}}{\cos\frac{\theta}{2}}.$$

From the figure $\tan\alpha = \frac{3\cdot5}{23}$, $\tan\beta = \frac{3\cdot5}{193}$.

$$\therefore \alpha = 8^\circ\ 39'\ 9'' \qquad \beta = 1^\circ\ 2'\ 20'' \qquad \theta = 7^\circ\ 36'\ 49'',$$

and
$$\frac{V}{V'} = 0\cdot99863 \equiv 1 - 0\cdot00137.$$

Thus the true velocity V is less than the velocity calculated in the General Table by 0·137 per cent. of the velocity in the Table.

This amounts to—

For the mean of Class	A, viz.,	1732 metres,	the correction is	2·37 metres.
" "	B, "	1775 "	"	2·43 "
" "	C, "	1942 "	"	2·66 "
" "	D, "	2013 "	"	2·76 "

Suppose the firing point distant 1·5 metre out of line, a similar calculation shows the correction to the velocity in the Table to be 0·00026 of that velocity or 0·026 per cent.

For the mean velocity of Class	A	this correction is	0·45 metre.
" "	B	"	0·46 "
" "	C	"	0·50 "
" "	D	"	0·52 "

It is probable that in general the firing buoy was not anything like a metre out of line, and hence it is clear that it is useless to apply this correction to the observed velocities. In any case a glance at the Table will show that the irregularities observed are of such an order as render any attempt to adjust the observations in this respect of purely fictitious value.

Discussion of Results.

It will be convenient to collect here the main results of the investigation as far as the comparison with the velocity of sound is concerned.

Table of Comparison.

Class.	Description of explosive.	Number* of experiments (complete).	Temperature at which comparison is made.	Velocity found (probable).	Velocity of sound calculated.	Excess of velocity found over velocity of sound.
				met. per sec.	met. per sec.	per cent.
A........	9 oz. of dry guncotton	11	17·79° C.	1732 ± 22	1523	13·75
B........	10 oz. Nobel's No. 1 dynamite	24	14·5° C.	1775 ± 27	1508	17·7
C........	18 oz. dry guncotton	5	18·3° C.	1942 ± 8	1525	27·3
D	64 oz. dry guncotton	3	19° C.	2013 ± 35	1528	31·7

Though the regularity of the mean results is very satisfactory, a glance at any of the Tables will show that several individual observations deviate to the extent of nearly $\frac{3}{4}$ per cent. Now from our apparatus, and from the fact that the observations are to a great extent made automatically, we are at a loss to account for these deviations, unless they are real. We hope to be able to show the cause of this immediately, for the moment we wish to add a little to what has already been said as to the rejection of certain observations. When we first began to get readings our gauges were not nearly so satisfactory as they afterwards became; the sensitiveness was sometimes so small that we occasionally failed in obtaining any record whatever from the apparatus which was furthest away from the firing point. The gauges were also apt to allow water to leak in at the bottom, and the air sometimes escaped slightly at the top, allowing the rubber faces to collapse. Both these accidents tend to lessen the sensitiveness of the gauges very materially. It was soon noticed that the abnormal results—and these always occurred in the direction of making the velocity too small—were obtained when the gauges were for some reason or other more or less out of order. When, as shortly occurred, we succeeded in making the gauges uniformly sensitive, and preventing water-logging and the escape of air, we got no abnormal times. The only difficulty we experienced in deciding to reject certain obser-

* Each experiment requires two separate explosions and time-measurements.

vations lay in the fact that at first we could see no reason why, if the gauges worked at all, they should give results which differed. We could not see that the introduction of water, for instance, between the rubber faces ought to make the time-constant of the gauge greater—and yet at first we could only explain the abnormal low velocities by supposing that the time-constants were enormously increased at the further stations. A fortunate observation, however, on September 13th, put us on the right track. We had noticed on several occasions that the first mark made by the scriber on the smoked plates was not nearly so strong as the second or third. These marks did not depend in any way on the natural period of vibration of the scriber, for that was far too short, and hence we put them down to the passage up and down the air-tube of the gauge of pressure waves. It was not yet clear, however, why it was that sometimes the second and third marks of the scriber were stronger than the first.

The strength or distinctness of these marks clearly depends on the conductivity and duration of the contact effected in the gauge head; and this must depend on the increase of pressure and on its duration in the water in the neighbourhood of the rubber disks. Now it is probable that several waves and not a single wave of compression start from an explosive centre. First there is the sudden expansion by the explosion, then the cooling or escape of the gases and the consequent falling together of the water before the disturbance subsides.

A glance at the map will show that our gauges were so situated with respect to the sheer stone wall of the quay that interference might well be looked for. Supposing then that interference takes place at the further gauge between the direct and reflected waves; it is clear that the first pressure to which the gauge is exposed is not necessarily the strongest, and equally clear that the duration of the interference pressure may be longer, but cannot very well be shorter than that of the pressure first arriving. It may well happen, therefore, that the second or third marking on the plate is the most distinct, or, even if the sensitiveness of the gauge is low, the first mark might be suppressed entirely, and the second appear alone in its stead. Now, the distance from the first to the second marking on the plate was pretty constant so long as the conditions of the experiment remained the same, and, strangely enough, as it then seemed to us in the case of the abnormal results referred to, there was also a certain agreement. It seemed as if the velocity was either a good deal greater than the velocity of sound—or else a good deal less; the abnormal velocities were consistent with themselves in a rough way. On September 13th, however, we found out the reason—on that day we got a record in which the first motion of the scriber (giving a normal time) was all but too small for detection—

while a little further on there was a strong mark giving one of the usual abnormal times.

The result was that we obtained a normal observation, but we saw that had the gauge been slightly less sensitive, or the explosion slightly smaller, we should have had an abnormal observation.

In order to obtain every satisfaction on this point, we applied (during the reduction of the observations) the mathematical criteria of rejection, and found that we should thus reject those observations —and those only which we had already decided to reject on experimental grounds. Altogether we rejected three experiments, involving six shots, out of forty-six experiments involving ninety-two shots.

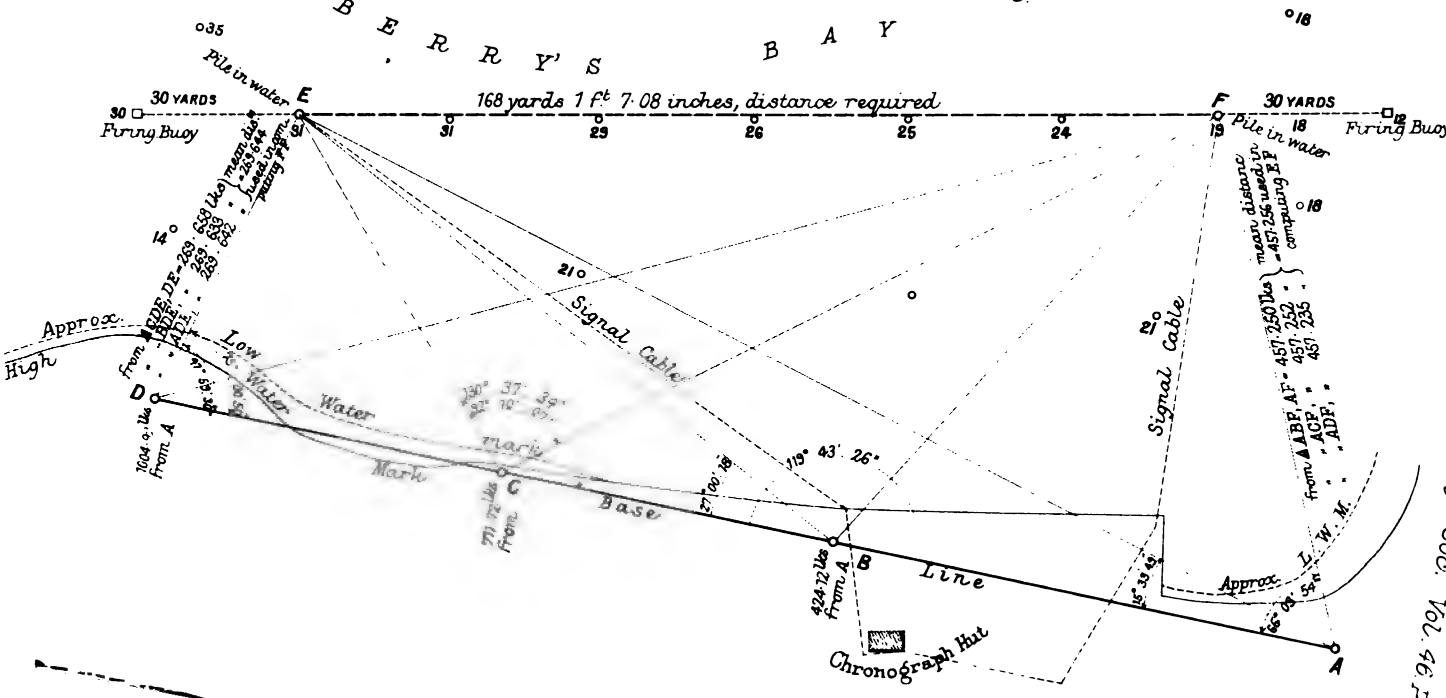
We now pass on to discuss the smaller variations in the observations which we retain. It will be remembered that at first our primary object in undertaking this work was to find whether there were any great irregularities or not, with a view to finding whether the "directed action" already commented on existed to any great extent. If it did, we hoped to find considerable deviations in the velocity, principally above the mean.

The result has shown, however, that while such irregularities do certainly exist to a slight extent, we have no evidence to show that directed action takes place from the explosion of freely suspended cartridges. The result would probably have been different if we had endeavoured to produce such directed action by the application of properly disposed obstacles or air spaces round the detonating point. We think that the small deviations, such as they are, are real, and do not depend on the gauges or chronograph, and certainly not on our measurement of the plates, which was always pushed to a much higher degree of certainty. It seems probable to us that though the directed action is small, it exists to some extent. The observations themselves afford strong proof of the fact that waves of great energy travel faster than sound, and, indeed, in free water, at all events (and, of course, we have no opinion beyond), the velocity is greater the greater the amount of energy transmitted. If it happens, therefore, that in any experiment the direction of greatest action coincides with the line passing from the shot through the two stations, we shall have that explosion register an abnormally high velocity. On the contrary, if the energy escapes away from the piles, we shall get a velocity correspondingly low. In intermediate directions of escape, the velocity will be intermediate.

A somewhat delicate question presents itself as to the precise method by which these big waves may be considered to become extinguished. A preliminary question which might very well be asked would be as to whether the increase in the velocity is to be looked for as depending on a greatly increased adiabatic resilience. We do not think that any probable change in the resilience would

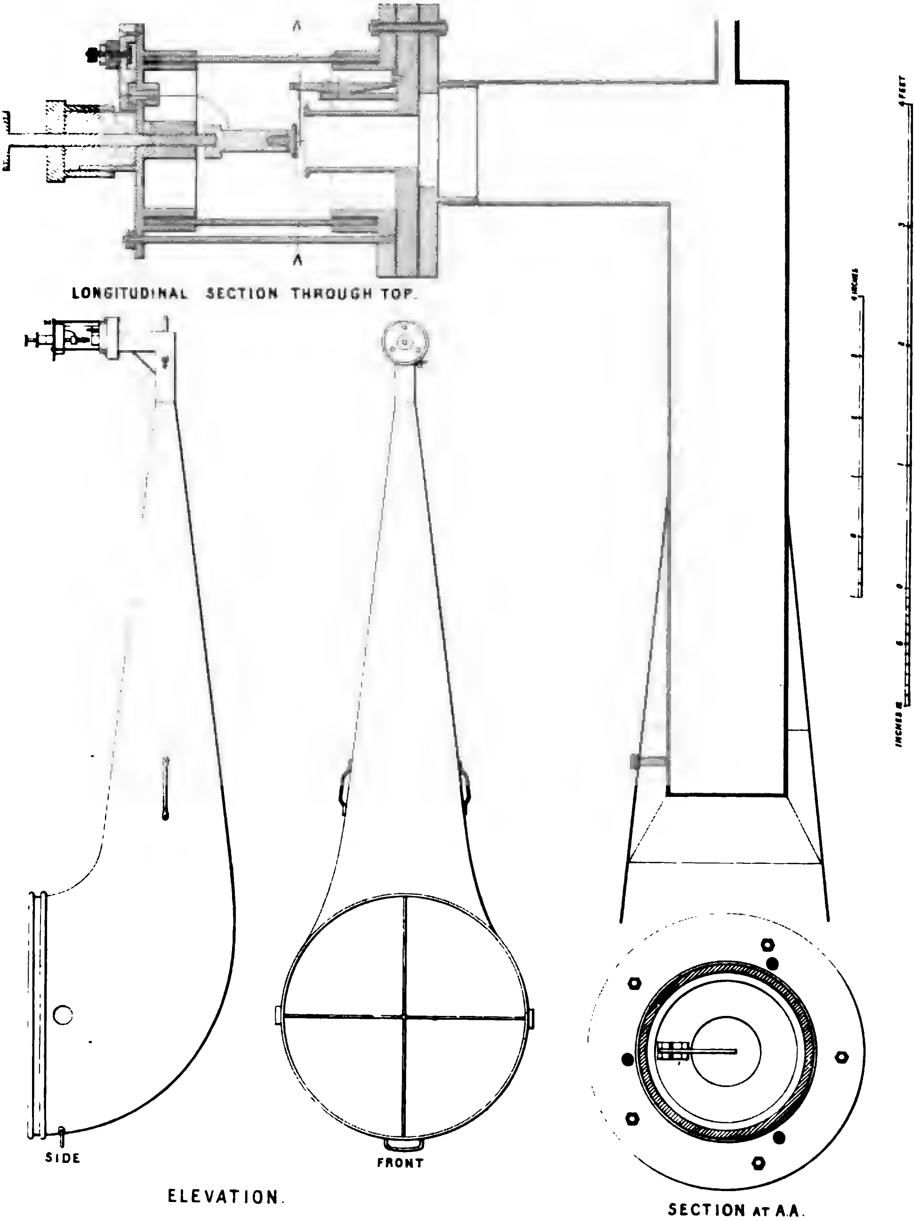
...owing method for finding the distance E F between two spikes in the tops of two piles in Berry's Bay, Port Jackson.

The numbers show the depth of the Water in feet at half tide.



...etfall & Adair.

...n, lith.



West, Newman, lith.

account for our results—nor, indeed, would this view be consistent with the remarks made on the subject of choosing Tait's formulæ. On the other hand, we have reasons for thinking that after the pressure increases beyond a certain point, the resilience may increase with very great rapidity; if this is the case, it will explain our results, the wave would rush past the first gauge, and then slow down with comparative suddenness.

It may be remembered that with respect to Class A, there was some evidence in favour of thinking that the velocity depends largely on the temperature of the water; this conclusion was not borne out by Class B, where the temperature was slightly higher. Without pretending to say that the evidence advanced is of any real importance, depending as it does on single observations encumbered with their private peculiarities, we may note that it would not be at all unlikely for velocities measured as we measured these to have large positive temperature coefficients. There is every reason to suppose that under the conditions of our experiments the viscosity of the water will be an important factor in determining the rate of decay of the disturbance as it is propagated outwards. Now, of all the physical properties of water, viscosity is the one which varies most rapidly with temperature, and, consequently, it is not unlikely that the decay of amplitude, and hence velocity in the disturbance, may depend to a great extent on the temperature.

In addition to the wave of great amplitude whose velocity has formed the subject of this paper, there are, in all probability, waves of varying degrees of amplitude and velocity resulting from the explosion. These waves, together with the final group, having practically the velocity of sound do not, at present, present any features of particular interest

General

1. Date of observation.	2. Nature and quantity of explosive.	3. Temperature of the fork.	4. Temperature of the water.	5. Distance between tops of piles.	6. Correction due to slope of pile.
May 8	9-oz. disk of gun-cotton.	15·1° C.	17·6° C.	cm. 15410	0
"	"	"	"	"	"
May 11	"	"	"	"	"
"	"	"	"	"	"
May 16	"	"	17·5	"	"
"	"	"	"	"	"
May 18	"	"	17·0	"	"
"	"	"	"	"	"
May 19	"	"	"	"	"
"	"	"	"	"	"
Sept. 21	"	23°	18·5	18210	"
"	"	"	"	"	"
"	"	21	"	"	"
"	"	"	"	"	"
Sept. 23	"	25	18°	"	"
"	"	"	"	"	"
"	"	"	"	"	"
"	"	"	"	"	"
"	"	24	"	"	"
"	"	"	"	"	"
"	"	"	"	"	"
"	"	"	"	"	"
July 5	Dynamite, 10 oz.	16	14·5°	15840	−51
"	"	"	"	"	"
July 11. Left to right.	"	15·5°	14·8	"	−51
" Right to left.	"	"	"	"	"
" Left to right.	"	"	"	"	"
July 26	"	13·4	13°	"	−59
"	"	"	"	"	"
"	"	"	"	"	"
"	"	"	"	"	"
Aug. 1	"	14·5	13·9°	"	"
"	"	"	"	"	"
Aug. 3	"	13·5	13·3	"	"
"	"	"	"	"	"
"	"	"	"	"	"
"	"	"	"	"	"
"	"	13·6	"	"	"
"	"	"	"	"	"
"	"	"	"	"	"
"	"	"	"	"	"
Aug. 10. Left to right	"	13·4	13·5	"	"
" Right to left	"	"	"	"	"
" Left to right	"	"	"	"	"
"	"	13·5	13·4	"	"
"	"	"	"	"	"
"	"	"	"	"	"
"	"	"	"	"	"
Aug. 25	"	18·8	15·5	18210	0
"	"	"	"	"	"

Table.

7. Observed time interval.	8. Mean of two intervals.	9. Uncorrected velocity.	10. Correction due to fork.	11. Velocity corrected for temperature of fork.	12. Calculated velocity.
0·063 0·121	0·0920	167500	+67	167567	152196
0·0768 0·1090	0·0929	165877	+66	165943	„
0·0600 0·1360	0·0980	157245	+63	157308	152152
0·0754 0·1200	0·0977	157728	+63	157791	151934
0·0739 0·1332	0·1036	148745	+59	148804	„
0·0885 0·1129	0·1007	180834	−137	180697	152589
0·0888 0·0989	0·0939	193930	−91	193839	„
0·0969 0·0869	0·0919	198150	−210	197940	152371
0·1348 0·0877	0·1113	163612	−173	163439	„
0·0908 0·0933	0·0921	197720	−180	197540	„
0·1150 0·0931	0·1042	174760	−159	174601	„
0·0769 0·1243	0·1006	156948	+42	156990	150807
0·1310 0·0800 0·0800	0·0928	170140	+58	170198	150943
0·0810 0·1305	0·1058	149159	+97	149256	150117
0·0820 0·1280	0·1050	150295	+98	150393	„
0·0833 0·0800	0·0817	193158	+94	193252	150533
0·1040 0·1009	0·1025	153961	+98	154059	150256
0·1028 0·1009	0·1019	154867	+98	154965	„
0·1081 0·0991	0·1036	152326	+94	152420	„
0·1053 0·0983	0·1018	155020	+96	155116	„
0·1005 0·0803 0·0990	0·0901	175150	+114	175264	150348
0·0656 0·0994	0·0825	191285	+121	191406	150302
0·0686 0·0987	0·0837	188542	+120	188662	„
0·0799 0·0897	0·0848	214741	−31	214710	151263

General

1. Date of observation.	2. Nature and quantity of explosive.	3. Temperature of the fork.	4. Temperature of the water.	5. Distance between tops of piles.	6. Correction due to slope of pile.
				cm.	
Aug. 25	Dynamite, 10 oz.	18·8	15·5°	18210	0
,, ...	,,	,,	,,	,,	,,
,, ...	,,	18°	,,	,,	,,
,, ...	,,	,,	,,	,,	,,
,, ...	,,	,,	,,	,,	,,
,, ...	,,	,,	,,	,,	,,
Sept. 10	,,	15	15·2	,,	,,
,, ...	,,	,,	,,	,,	,,
,, ...	,,	,,	,,	,,	,,
,, ...	,,	,,	,,	,,	,,
,, ...	,,	17·5°	,,	,,	,,
,, ...	,,	,,	,,	,,	,,
,, ...	,,	,,	,,	,,	,,
,, ...	,,	,,	,,	,,	,,
Sept. 13	,,	18·6	16°	,,	,,
,, ...	,,	,,	,,	,,	,,
,, ...	,,	,,	,,	,,	,,
,, ...	,,	,,	,,	,,	,,
,, ...	,,	21·8	15·8°	,,	,,
,, ...	,,	,,	,,	,,	,,
,, ...	,,	,,	,,	,,	,,
,, ...	,,	,,	,,	,,	,,
Sept. 23	Guncotton, 18 oz.	23°	18°	,,	,,
,, ...	,,	,,	,,	,,	,,
,, ...	,,	,,	,,	,,	,,
,, ...	,,	,,	,,	,,	,,
Oct. 3	,,	28	18·5°	,,	,,
,, ...	,,	,,	,,	,,	,,
,, ...	,,	,,	,,	,,	,,
,, ...	,,	,,	,,	,,	,,
,, ...	,,	25·5°	,,	,,	,,
,, ...	,,	,,	,,	,,	,,
Oct. 7	Guncotton, 4 lbs.	26°	19°	,,	,,
,, ...	,,	,,	,,	,,	,,
,, ...	,,	,,	,,	,,	,,
,, ...	,,	,,	,,	,,	,,
,, ...	,,	23	20°	,,	,,
,, ...	,,	,,	,,	,,	,,

Table—(*continued*).

7. Observed time interval.	8. Mean of two intervals.	9. Uncorrected velocity.	10. Correction due to fork.	11. Velocity corrected for temperature of fork.	12. Calculated velocity.
0·0980 0·0879	0·0929	196017	−28	195989	151263
0·0807 0·0904	0·0856	212734	−6	212728	"
0·0927 0·0909	0·0918	198366	−5	198361	"
0·0912 0·0899	0·0906	200993	+83	201076	151128
0·0906 0·1109	0·1008	180655	+75	180730	"
0·0932 0·0898	0·0915	199016	+9	199025	"
0·0933 0·1096	0·1015	179409	+8	179417	"
0·1019 0·0933	0·0976	186578	−22	186556	151487
0·1210 0·1343	0·1277	142600	−17	142583	"
0·0946 0·0948	0·0947	192291	−113	192178	151397
0·0950 0·1133	0·1042	174760	−103	174657	"
0·0928 0·0915	0·0922	197505	−151	197354	152371
0·0918 0·0906	0·0912	199671	−152	199519	"
0·097264 0·091394	0·09433	193046	−290	192756	152589
0·097555 0·090227	0·09389	193950	−291	193659	"
0·101838 0·091994	0·09692	187887	−213	187674	"
0·079074 0·094150	0·08661	210253	−253	210000	152802
0·097445 0·091448	0·09445	192800	−232	192568	"
0·097873 0·091343	0·09461	192474	−147	192327	153228

Other Observations from which Mean Intervals of Time

1. Date of observation.		2. Nature and quantity of explosive.	3. Temperature of the fork.	4. Temperature of the water.	5. Distance between tops of piles.	6. Correction due to slope of pile.
					cm.	
April 25	....	Guncotton, 9 oz.	15·1°	17·6°	15410	0
,,	....	,,	,,	,,	,,	,,
May 7.	Left to right.	,,	,,	,,	,,	,,
,,	Right to left.	,,	,,	,,	,,	,,
,,	Right to left.	,,	,,	,,	,,	,,
July 5	....	Dynamite, 10 oz.	16°	14·5	15840	−51
,,	....	,,	,,	,,	,,	,,
July 7.	No record on far gauge	,,	..	..	..	..
July 11.	No record on gauge	,,	..	..	..	..
July 13.	No record...	,,	..	..	..	..
,,	....	,,	16	14·3	15840	−51
,,	No record...	,,	..	..	..	..
,,	No record...	,,	..	..	..	..
,,	No record...	,,	..	..	..	..
,,	No record...	,,	..	..	..	..
July 26.	....	,,	13·4°	18°	15840	−59
,,	Record lost by smudging	,,	..	..	..	..
Aug. 1.	No record...	,,	..	..	..	..
,,	No record...	,,	..	..	..	..
,,	No record...	,,	..	..	..	..
Aug. 10.	Missfire....	,,	..	..	..	..
Sept. 13.	Gauges readjusted	,,	18·6	16	18210	0

could not be obtained, and the Rejected Observations.

7. Observed time interval.	8. Mean of two intervals.	9. Uncorrected velocity.	10. Correction due to fork.	11. Velocity corrected for temperature of fork.	12. Calculated velocity.
0·1301 } 0·1294 }	0·1298				
0·1632 } 0·1169 }	0·1401				
0·1176	..				
0·1508 } 0·1268 }	0·1388				
..	..				
..	..				
..	..				
0·0813	..				
..	..				
..	..				
..	..				
..	..				
0·1015	..				
..	..				
..	..				
..	..				
..	..				
..	..				
0·1142	..				

"The Ferment-action of Bacteria." By T. LAUDER BRUNTON, M.D., F.R.S., and A. MACFADYEN, M.D., B.Sc.

Received March 23,—Read April 4, 1889.

In the course of the research the following micro-organisms were used :—

1. Koch's comma spirillum (Flügge, 'Die Mikro-organismen,' Leipzig, 1886, p. 334).
2. Finkler's comma spirillum (Flügge, 'Die Mikro-organismen,' Leipzig, 1886, p. 382).
3. A putrefactive micrococcus.
4. Scurf bacillus (Klein).
5. A bacillus isolated from milk by Dr. Klein, which for convenience we may call the "Welford Bacillus."

All of these liquefy gelatine, the two last most energetically. Anthrax was not used, on account of the resistance of its spores and the consequent difficulty of completely sterilising the culture media. The experiments were made in each case with pure cultures.

The first question which we tried to solve was, What is the nature of the substance by which bacteria liquefy gelatine? Is it an enzyme? There are two ways in which they might do this. They might secrete some fluid which would dissolve the gelatine mechanically, without altering it chemically, as saliva dissolves sugar in the mouth; or they might do it by secreting a specific enzyme, which would dissolve the gelatine by altering it chemically, as the ptyalin of the saliva effects the solution of starch. If the solution were effected in the first way by the secretion of a mere solvent, we should expect that when the microbes were removed or destroyed, either by heat or chemical means, the portion of the medium already dissolved would not have any extensive action on fresh media. But if it had any such solvent action, it would probably continue after the solution had been heated to a temperature sufficient to destroy the action of an enzyme. If, on the other hand, the microbes liquefied the media by secreting an enzyme, we should expect that the liquefied portion would probably dissolve a considerable amount of new medium when added to it, but that its solvent action would be arrested by exposure to a temperature sufficient to inhibit enzyme action.

The culture medium was made by adding to meat broth: gelatine, 10 per cent.; peptone, 1 per cent.; and sodic chloride, 0·5 per cent. The reaction was rendered faintly alkaline with carbonate of soda. In all the experiments Koch's methods to ensure sterile media and pure cultures were followed out.

Tubes of 10 per cent. gelatine were inoculated with the five microbes, and placed in the incubator at 37° C., with the exception of the putrefactive micrococcus, which was kept at 22° C.

When liquefaction was complete the fluid was filtered into sterile tubes, the bacterial deposit being washed with a small quantity of sterile distilled water.

Of the filtrate, one, three, and five drops were added respectively to fresh gelatine, and the tubes placed in the incubators as before. The gelatine liquefied, and in all cases bacteria were present.

This liquefied gelatine was in its turn taken and subjected to a temperature of 50° C. for one hour. Then one, three, and five drops were added to fresh gelatine. After incubating, some of the cholera comma tubes did not liquefy, but in all cases where liquefaction took place it was due to the active bacteria, as proved by their growth on control plates. The control plates were made by adding a few drops of the liquefied gelatine to fresh gelatine, and pouring it out in a sterile glass dish. After incubating at 22° C., the gelatine was examined microscopically, and the presence or absence of bacterial colonies noted.

The liquefied gelatine was next subjected to a temperature of 100° C. for fifteen minutes. The same number of drops were added to gelatine. This fresh gelatine did not liquefy. Finally, 5 c.c. were added to fresh gelatine, but still it did not liquefy.

The control plates showed no colonies.

We therefore conclude that exposure to a temperature of (I) 100° C. destroys—

(*a.*) The bacteria.

(*b.*) The liquefying power of the fluid.

(II) 50° C. does neither. It was not deemed advisable to continue the sterilisation too long, having regard to the injurious action of heat on soluble ferments.

It was next necessary to determine the temperatures between 50° C. and 100° C., which would be sufficient to kill the bacteria without rendering any ferment which might exist inactive. A series of experiments led to the following results:—

60° C. for half an hour killed Koch's and Finkler's spirillum.

75° C. for fifteen minutes, on two successive days, killed the scurf and "Welford" bacilli.

70° C. for fifteen minutes, on two successive days, destroyed the putrefactive micrococcus.

Having established these facts, a series of cultures at 37° C. were made in small glass flasks, each containing about 100 c.c. of 10 per cent. gelatine. The liquefied gelatine was filtered, and the deposit washed with sterile distilled water.

These filtrates from the five series of cultures were sterilised as described above. Then 5—10 c.c. of each were added to 10 per cent. gelatine (20 c.c.) and kept at 37° C., as well as control tubes of sterile gelatine.

On the third day the tubes were removed from the incubator and placed in ice-cold water.

Results:—

Scurf bacillus Welford bacillus	The gelatine does not stiffen, but remains liquid.
Koch's spirillum Finkler's spirillum	The gelatine is semi-liquid, and does not completely re-gelatinise.
Putrefactive micrococcus Control gelatine	The gelatine stiffens.

Control plates. No bacteria.

Kept at the ordinary room temperature, these phenomena persisted, the liquid gelatine remaining liquid, and the solid gelatine not liquefying.

Here, then, we have complete liquefaction of the gelatine produced in the first two cases, partial liquefaction in the next two, and no effect in the last.

That this liquefaction was brought about without the presence of active bacteria is proved by the fact that control plates inoculated from the liquefied gelatine remained sterile. The complete liquefaction was produced by the sterile fluid from the microbes which were more active liquefiers of gelatine than the others. In the case of the two comma spirilla the enzyme action in gelatine was evidently more feeble. The negative result with the putrefactive micrococcus, and also the fact that tubes inoculated from it, and kept at the optimum temperature of 22° C., also gave negative results, were probably due to the preliminary sterilisation having destroyed both the microbes and any enzyme which they might have formed.

These introductory experiments led to the following conclusions:—

1. 100° C. destroyed both the bacteria and the liquefying power.

2. 50° destroyed neither the bacteria nor the liquefying power.

3. Temperatures between 60° and 75° C. destroyed the bacteria, but not the liquefying power in four cases.

4. The liquefied gelatine treated as under 3, and added to fresh gelatine, liquefied it, although active bacteria were proved to be absent.

5. The liquefaction must, we think, be due to a soluble enzyme, inasmuch as liquefaction still took place after the elimination of the microbes, while it was prevented by exposure to such a temperature as would destroy the activity of an enzyme but would not be likely to affect the action of a simple solvent.

II.

Having regard to the fact that the peptonising action in gelatine was slow, and in two cases partial, it was next sought to determine whether more active liquefaction of the gelatine could be obtained by growing the microbes in some other albumenoid soil.

Two culture fluids were made with meat broth as follows:—

A. Meat broth—	B. Meat broth—
Peptone, 1 per cent.	NaCl 0·5 per cent.
NaCl 0·5 "	

Both were rendered faintly alkaline with the carbonate of soda.

The bacteria grew well in both of these media, and so rapidly and abundantly in B. that further experiments were made with it only, *i.e.*, without peptones. For each culture, 100 c.c. meat broth were used. After inoculation and four days' incubation at 37° C., the broth was filtered, and the bacterial deposit washed with sterile distilled water. It was then sterilised as already described, and 10 c.c. added to tubes of 10 per cent. gelatine. These tubes were placed in the incubator, as well as control tubes of sterile gelatine. When taken out, and placed in ice-cold water, the following results were obtained:—

(1.) After 24 hours:

Scurf bacillus
Welford bacillus } Liquid.

Koch's spirillum
Finkler's spirillum } Semi-liquid.

Putrefactive micrococcus
Control gelatine } No liquefaction.

Control plates. No colonies.

(2.) After 48 hours:

Koch's spirillum
Finkler's spirillum } Liquid.

Putrefactive micrococcus
Control gelatine } No liquefaction.

Control plates. No growth.

From these experiments it will be seen that the enzyme developed in meat broth is more active than that formed in gelatine. In twenty-four hours the gelatine was liquefied by the scurf and Welford bacilli; in forty-eight hours by Koch's and Finkler's comma spirilla. Again the putrefactive micrococcus gave negative results.

Conclusions:—

1. An enzyme is formed in meat broth which liquefies gelatine,

and does so more surely and quickly than the enzyme formed in gelatine itself.

2. The liquefaction is produced by a soluble ferment, since its action can be demonstrated apart from the microbes which produce it.

III.

Instead of using heat sterilisation some experiments were made with menthol and thymol.

It was found that when these substances were added in amounts sufficient to prevent the growth of the bacteria—the ferment action was likewise inhibited.

IV.

The presence of a soluble ferment being demonstrated, can we isolate it?

(1.) From gelatine.

(2.) From meat broth.

(1.) *From Gelatine Cultures.*

Flasks containing 250 c.c. of 10 per cent. gelatine were inoculated with the five microbes. They were left in the incubator at 47° C., (putrefactive micrococcus, 32° C.), till liquefaction was complete. The liquefied gelatine was treated with absolute alcohol and filtered. The precipitate was extracted with glycerine, and finally reprecipitated with alcohol. The precipitate, after drying in a sterilised flask, was taken up in a small quantity of sterile distilled water, and allowed to stand over night. About 5 c.c. were then added to 10 per cent. gelatine, and incubated at 37° C.

Results.—Negative. No liquefaction was produced.

(2.) *Meat Broth Cultures.*

In each case 250 c.c. were treated in a similar manner—with alcohol and glycerine, and the precipitate and sterile distilled water added to 10 per cent. gelatine.

Results:—

Koch's spirillum Finkler's spirillum Putrefactive micrococcus	No liquefaction.
Scurf bacillus Welford bacillus	In a few tubes the gelatine was viscid. The rest resolidified.
Control plates.	No colonies.

Concluding that the prolonged method of extraction had weakened

the action of the enzyme, a modification of the process was next made in the following manner:—500 c.c. of meat broth were inoculated with the microbes, and left in the incubator for seven days. The precipitate, with an excess of alcohol, was allowed to stand overnight, and, after drying, was dissolved in sterile distilled water, and then reprecipitated by alcohol. This precipitate was dried and taken up in distilled water. The next day about 20 c.c. were added to 100 c.c. of a 5 per cent. gelatine, and placed in the incubator at 37° C.

Results after four days:—

The only positive results were obtained with the scurf bacillus and the Welford bacillus. In these cases the gelatine remained liquid, while the control gelatine resolidified. The control plates gave no colonies.

Conclusion.—The bacteria do form a soluble enzyme which can be isolated, and its action demonstrated on albumenoid gelatine.

V.

Are the microbes which liquefy gelatine capable of exerting a like action on other proteid bodies?

To test this, experiments were made with—

(*a.*) Egg-albumen.
(*b.*) Fibrin.

In the first place, it was necessary to find out what resulted from the direct action of the microbes.

Faintly alkaline meat broth, as developing the most active enzyme, was used.

(a.) *Egg Albumen.*

To flasks containing 100 c.c. of meat broth were added small pieces of coagulated egg albumen. The flasks were then sterilised and inoculated with Koch's spirillum, Finkler's spirillum, the scurf and Welford bacilli. They were then placed in the incubator at 37° C.

Results:—

(1.) Scurf bacillus.
Welford bacillus:—

1st day. No marked change.
2nd day. Albumen broken up into small fine flocculent fragments.
3rd day. Disintegration almost complete.
4th day. Disintegration complete.

(2.) Koch's spirillum.
Finkler's spirillum:—
1st day. No marked change.
2nd day. Translucent.
3rd day. Thinned and transparent.
5th day. Disintegration.

The bacteria are therefore able, by means of their peptonising action, to disintegrate egg albumen.

(b.) *Fibrin.*

To 100 c.c. of the meat broth small pieces of boiled fibrin were added, and after sterilisation the flasks were inoculated with the same microbes, then placed in the incubator at 37° C.

Results:—

(1.) Scurf bacillus.
Welford bacillus.
1st day. No marked change.
2nd day. Fibrin eroded.
3rd day. Breaking up.
4th day. Disintegration complete.
5th day. Fluid has become turbid.

(2.) Koch's spirillum.
Finkler's spirillum:—
1st day. No change.
2nd day. Slight erosion.
3rd day. Frayed appearance.
4th day. Commencing to break up.
5th day. Disintegrated.
6th day. Turbidity.

Here again we have a marked disintegrating action on fibrin.

Conclusion.—The bacteria exert a disintegrating action on egg albumen and fibrin, as well as on gelatine.

VI.

Can we demonstrate the action of the enzyme on proteid bodies such as egg albumen and fibrin, in the same way that its action was demonstrated on gelatine?

The alcoholic precipitate from 500 c.c. of the meat broth culture was dried at 35° C., and then dissolved in sterile distilled water. It was then reprecipitated by alcohol and filtered. This precipitate was dried in sterile plugged flasks, and to it were added 50 c.c. of sterile

distilled water, and 5 c.c. of a $\frac{1}{2}$ per cent. chloroform water. Carbonate of soda was finally added to render the fluid faintly alkaline.

In each flask was placed a small piece of boiled fibrin. After four days in the incubator they were taken out and examined :—

A. From each, gelatine plate cultures were made.
B. The appearance of the fibrin was noted.
C. After filtration the filtrate was tested for digestive products.

A. Some of the plates showed bacteria. The flasks from which these had been made were rejected; only those were used which had remained sterile.

B. In none did the fibrin break up and disappear. But it became thinned and frayed at the edges. This was most marked with the scurf and Welford bacilli.

C. The filtrate was examined for soluble products : —

On neutralising with dilute hydrochloric acid a precipitate appeared. This was filtered off and the filtrate tested for peptones. A solution of caustic soda was added, and then a highly dilute solution of cupric sulphate was filtered down the side of the test tube. At the line of demarcation the rose-coloured peptone reaction was strongly marked.

The simple boiled solution of the ferment only gave the faintest peptone reaction.

These results were obtained with the scurf and Welford bacilli, and Koch's and Finkler's spirillum. To sum up :—

1. The fibrin was visibly affected.
2. Neutralisation produced a precipitate.
3. The peptone reaction was very distinct.

The enzyme therefore, apart from the bacteria, *can* form soluble products from fibrin, and amongst these peptones.

VII.

Are the microbes capable of forming a diastatic, as well as a peptonising ferment?

A. Scurf bacillus.
   Welford bacillus :—

Starch was heated with water so as to form a thin paste. To this was added sodic chloride (0·5 per cent.). About 100 c.c. were placed in each flask, which was then plugged with cotton wool and sterilised.

After inoculation they were placed in the incubator (37° C.) along with flasks of sterile starch paste.

Flasks were opened on successive days and examined :—

2nd day. Starch has lost its opalescence. Iodine gives a blue colour.
3rd day. Iodine gives a red colour.
5th day. No reaction with iodine.
6th day. Was tested for a reducing sugar. The reactions were as follows:—

(1.) Iodine.—No reaction.
(2.) Caustic soda.—On gently boiling fluid becomes yellow.
(3.) Cupric sulphate and caustic soda.—A yellow precipitate on boiling.
(4.) Fehling's reagent.—A red precipitate.
(5.) Barfoed's reagent.—No reaction on gently heating.

(*Barfoed's Solution*:—One part of neutral acetate of copper dissolved in 15 parts of water, and then to 200 c.c., 5 c.c. of acetic acid (38 per cent.) added.)

The control starch gave blue colour with iodine, but none of the above reactions.

B. Putrefactive micrococcus—
Results were negative.
C. Koch's spirillum.
Finkler's spirillum:—

The same starch solution was used, but a few drops of meat broth were added in each case. The usual control experiments were made:—

3rd day. Starch has lost its opalescence. Iodine strikes a blue colour.
4th day. Iodine gives a violet colour.
5th day. Iodine gives red reaction.
7th day. Iodine.—Red.
Caustic soda.—Yellow on boiling.
Cupric sulphate and caustic soda.—No reduction.
Fehling's solution.—No reduction. On previous addition of H_2SO_4 a slight reduction.
Barfoed's reagent.—No reduction.
Control starch.—Iodine strikes blue.

From these experiments the following conclusions may be drawn:—

1. The putrefactive micrococcus did not grow on the carbohydrate soil, and so we are left in doubt as to its diastatic action.

2. The scurf bacillus and Welford bacillus were both capable of cultivation, and evinced a marked diastatic action, in addition to their peptonising power. The failure of the iodine test, and the

precipitates obtained with Fehling, &c., indicate the presence of a reducing sugar. The failure with Barfoed's reagent suggests that the sugar is in great part, at any rate, maltose.

3. With regard to Koch's spirillum and Finkler's, though they evinced a diastatic action, it was feebler than in the former case, only traces of a reducing sugar being detected after the addition of sulphuric acid. The red and violet coloration with iodine points to the formation of dextrin (erythro- and achroo-dextrin).

At any rate, in the scurf and Welford bacilli we have two microbes which evince a marked diastatic action; and a demonstration of the fact that the same germ can produce both a diastatic and a peptonising ferment.

VIII.

Can we demonstrate the action of the diastatic enzyme apart from the bacteria?

Starch cultures of the scurf bacillus and the Welford bacillus (two days' growth) were treated with chloroform water (1 per cent.) till they became sterile.

The fluid was then added to fresh starch, and incubated at 37° C.

In eight to ten days the iodine reaction had disappeared. On boiling with caustic soda the fluid became yellow. Fehling's solution was reduced. The fluid lost its opalescence. Control plates—no growth.

These experiments point strongly to the existence of a diastatic enzyme capable of isolation, and of acting apart from the bacteria.

IX.

That the peptonising enzyme bears the closest analogy to the pancreatic ferment will be seen from the following experiments. Sterile meat broth, in which Finkler's spirillum and the Welford bacillus had been cultivated, was added to 10 per cent. gelatine tubes of differing reaction:—

Gelatine.	*Results.*
A. Acidified with dilute hydrochloric acid..	No liquefaction.
B. Alkaline by adding sodic carbonate	Liquefied.
C. Neutral	Liquefied.
D. Boiled after adding the ferment........	No liquefaction.

X.

The digestive action of the microbes was tested on several other bodies.

1. *Fats.*—Alkaline meat broth and olive oil, 2 per cent.

The results were negative.

Experiments which were made by Manfredi* tend to show that fat-containing media impair the vegetative energy of bacteria.

2. *Dextrose.*—The culture fluid was prepared as follows:—

Dextrose	2 per cent.
Peptone	1 „
Sodic chloride	0·5 „
Reaction	Neutral.

After sterilisation, the flasks were inoculated with the scurf bacillus and Welford bacillus. Incubated at 37° C. They were examined on the fourth day.

Fehling's solution was no longer reduced. The fluid gave a marked acid reaction.

The control solution reduced Fehling's solution. Reaction was unchanged.

3. *Cane-sugar.*—Cane-sugar, 2 per cent.
Peptone, 1 per cent.
NaCl, 0·5 per cent.
Reaction, neutral.

Inoculated with scurf bacillus and Welford bacillus, and incubated at 37° C.

The results were negative. No reducing sugar detected.

Muscle.—Alkaline meat broth cultures were used. Inoculated with Finkler's spirillum and Welford bacillus.

With the Welford bacillus a marked effect—the muscular tissue becomes disintegrated, and the striæ indistinct.

These experiments, though incomplete in themselves, are sufficient to show that the bacteria which liquefy gelatine and diastase starch, can also exert a digestive influence on dextrose and muscle. The exact determination of the products of this action in the case of these and some other organic bodies must be reserved for further investigation.

To sum up briefly the results of this inquiry:—

1. The bacteria which liquefy gelatine do so by means of a soluble enzyme.

2. This enzyme can be isolated, and its peptonising action demonstrated apart from the microbes which produce it.

3. The most active enzyme is that formed in meat broth.

4. Acidity hinders, alkalinity favours its action.

5. The bacteria which form a peptonising enzyme on proteid soil can also produce a diastatic enzyme on carbohydrate soil.

* 'Accademia dei Lincei, Rendiconti,' vol. 3, sem. 1, 1887, p. 535.

6. The diastatic enzyme is not so readily separated from the microbes which produce it, but where that has been accomplished its action on starch can still be demonstrated.

7. The diastatic enzyme has no effect on gelatine, and *vice versâ.*

8. The bacteria are capable of evincing an adaptiveness to the soil in which they grow.

9. The microbes are capable of digesting other similar bodies such as dextrose and muscle.

10. Fatty matter was not affected.

OBITUARY NOTICES OF FELLOWS DECEASED.

The Rev. Thomas Gaskin, who died at his residence at Cheltenham on February 17th, 1887, was born at Penrith, in Cumberland, in 1810. He was educated at Sedburgh, and for some time before he left was second in the School. On leaving Sedburgh School, in 1827, he proceeded to St. John's College. One of the men of his year writes that it was understood that he came to Cambridge under the auspices of Lord Brougham, who had formed a very high estimate of his mathematical talent, and was confident he would be Senior Wrangler. Later on, in 1851, Lord Brougham, writing a testimonial for Mr. Gaskin, speaks of an intimate acquaintance of above thirty years.

On entering St. John's College, he found the freshmen exceptionally strong in mathematics. The third wrangler of his year was then a Johnian, who afterwards migrated to Caius College. Besides the Senior, there was a fourth man reckoned equally good, whose health broke down under hard reading. Nevertheless, he was always placed first in the College examinations. This position could not be obtained, much less retained, every year, but by the exhibition of considerable classical knowledge. The late Dr. Kennedy, whose lectures he attended, entertained a favourable opinion of his promise as a classical scholar, though his chief attention was then devoted to mathematics. It has also been stated on the authority of some of his old schoolfellows, that on leaving Sedburgh he could repeat above twenty Greek plays by heart.

He took his degree of B.A. in 1831, as second wrangler and second Smith's prizeman, a member of his own College being Senior. The two were so nearly equal in merit that there was great difficulty in arranging their order. According to the custom of that day, cases of near equality were decided by questions given out orally, one by one, by some competent Master of Arts, not one of the Moderators or Examiners. In such a system a previous accidental acquaintance with the subject matter of a question would make an appreciable difference. Thus their positions in the final list, if decided by an insufficient number of questions, might indicate a greater difference than really existed.

Soon after his degree he was elected Fellow and Tutor of Jesus College, the duties of which responsible position he continued to discharge for eleven years. When, on his marriage, in 1842, he resigned these offices, a subscription was opened among his friends

and pupils, and he was presented with a valuable service of plate, as a testimony of their respect and esteem.

During his twenty years' residence in the University, he was almost uninterruptedly engaged in preparing pupils for the examination, and in writing books on subjects connected with their studies. He also during this time served the University in the difficult and responsible office of Proctor. He was six times chosen Moderator, viz., in the years 1835, 1839, 1840, 1842, 1848, and 1851. This is an appointment which no one before him had ever held so often. These repeated re-elections prove the general esteem in which his extraordinary power in constructing problems was held.

In 1836, Mr. Gaskin was elected a Fellow of the Royal Astronomical Society, and in 1839 he became a Fellow of the Royal Society.

He removed to Cheltenham about 1855, where he spent the remainder of his life. During his residence at this place, he occupied himself almost entirely with private pupils until his health failed. He here published a pamphlet on the theory and practice of solitaire. Subsequently, the broken state of his health, the early deaths of his sons and of his wife, made him choose a retired life, and prevented him from devoting his time to work. He seems never to have been a strong man, for he was an invalid even when Tutor of Jesus College.

Mr. Gaskin, when resident in Cambridge, was especially known for his unrivalled skill in the construction and solution of problems, especially such as required the application of complicated analysis. Indeed, we can see by his writings that this was the bent of his talents. In 1847, he published two volumes of "Solutions of Trigonometrical and Geometrical Problems." These were the solutions of examination papers set in St. John's College for the years 1830 to 1846. Though these books are of an elementary character, yet they must have had considerable influence on the studies of those seeking distinction in the examinations of that day. Even after this lapse of time, when so many new methods have come into use, a teacher would find here a useful collection of problems and examples with admirable solutions, a store-house from which he might draw material to lighten his own labours.

In the appendices, he added the solution of some other geometrical problems which were exciting interest at the time.

He also wrote in the 'Mechanic's Magazine,' and in 1848 published some papers on the inscription of polygons in the conic sections.

In Dr. Hymer's treatise on "Differential Equations," we find the solution of one of the problems proposed by him in the Senate House, when Moderator in 1839, given as the best and simplest

method of solving an important differential equation. This is the equation, a particular case of which occurs in the theory of the figure of the earth.

The problems and examples set in the Senate House are generally absorbed into the ordinary text-books, and become the standard examples by which successive generations of students acquire their analytical skill. On looking through Mr. Gaskin's papers, one cannot help noticing how many of his problems have been taken, and may be recognised as old friends. We thus learn one reason at least why his papers were so popular amongst the older writers. They are on all kinds of subjects, and generally are put in a way which shows that he was always on the look-out for a telling question. It seems to have been his custom to put any new theorem that he discovered in the form of a problem, rather than in that of a paper in a mathematical journal.

E. J. R.

Dr. Arthur Farre, the fifth son of the late Dr. John Richard Farre, was born in 1811, in the house in Charterhouse Square in which his father lived and practised for many years. He received his early education at the Charterhouse. In 1827 he became a pupil at St. Bartholomew's Hospital, and the following year he entered at Caius College, Cambridge. In the intervals of the University terms he prosecuted his medical studies at St. Bartholomew's. He was a diligent dissector, and as prosector under Abernethy, he prepared subjects for the last course of lectures on physiology delivered by that eminent surgeon. He graduated M.B. at Cambridge at the head of the medical list in 1833, and M.D. in 1841.

During 1836–7 Dr. Farre lectured on Comparative Anatomy at St. Bartholomew's Hospital, in succession to Mr. (now) Sir Richard Owen. From 1838 to 1840 he lectured on Forensic Medicine.

Dr. Farre contributed to the 'Philosophical Transactions' for 1837 an elaborate paper with numerous lithographic illustrations, entitled "Observations on the Minute Structure of the Higher Forms of Polypi, with views of a more natural Arrangement of the Class." The publication of this paper was followed by his election as a Fellow of the Royal Society in 1839. To the 'Philosophical Transactions' for 1843 Dr. Farre contributed a paper "On the Organ of Hearing in the Crustacea."

Dr. Farre's professional career was determined for him when, in the year 1842, he succeeded Dr. Robert Ferguson in the chair of Obstetric Medicine at King's College, and at the same time he was appointed Physician Accoucheur to King's College Hospital. These appointments he retained until 1862, when, on his retirement, he was made Consulting Physician.

He became a member of the College of Physicians in 1838, and was elected a Fellow in 1843. He was Censor in 1861–2, and Senior Censor in 1865. In 1872 he delivered the Harveian Oration, taking as his subject "Harvey's Exercises on Generation."

For a period of twenty-four years he held the appointment of Examiner in Midwifery at the Royal College of Surgeons.

Dr. Farre possessed all the qualities required in a great and successful obstetric physician, and he soon acquired a large practice amongst the very highest ranks. He attended the Princess of Wales at the birth of all her children. He also attended the late Princess Louis of Hesse Darmstadt, Princess Alice of Great Britain, in her first confinement; also Her Imperial Highness the Duchess of Edinburgh, in her first confinement at Buckingham Palace, in 1874, and again at Eastwell Park, in 1875. He attended Princess Christian in 1867, at Windsor Castle, and again in 1869, at Cumberland Lodge. He attended the Princess Mary Adelaide in all her confinements, and the Princess Leiningen in 1863, at Osborne. Few physicians have had the honour to attend successfully so many members of the Royal Family.

In 1875 Dr. Farre received the appointment of Physician Extraordinary to Her Majesty, and although no word of dissatisfaction ever escaped from him, his friends and professional brethren thought it strange that this should have been the only public royal recognition of his eminent services.

After the death of Sir Charles Locock, in 1875, Dr. Farre was made Honorary President of the Obstetrical Society.

His chief literary contribution to his own department of medicine was the elaborate article "Uterus and its Appendages," in Todd's 'Cyclopædia of Anatomy and Physiology.' Dr. Robert Barnes says of this article that it "is the fruit of remarkable labour and original research, digested and set forth with consummate judgment. To this day it stands, if not unrivalled, yet unsurpassed. It is rich in original illustrations, and just in acknowledging what is borrowed. It may be doubted whether any similar work has stood the trying test of time so well. Others may have added to it; few have made corrections that have held their ground."*

Dr. Farre was one of the founders of the Microscopical Society; he was its first honorary secretary, and served several times on its council, and in 1851–52 he was its President. Through the influence of the Prince of Wales he obtained for the Society its Royal Charter.

He communicated several papers to the Microscopical Society, amongst others the following:—"On the Minute Structure of certain Substances expelled from the Human Intestine, having the ordinary appearance of shreds of lymph, but consisting entirely of filaments

* 'Brit. Med. Journal,' Dec. 24, 1887.

of confervoid type, probably belonging to the genus Oscillatoria" ('Mic. Soc. Trans.,' 1, 1844); "An Account of the Dissection of a Human Embryo of about the fourth week of Gestation, with some observations on the early development of the Human Heart" ('Mic. Soc. Trans.,' 3, 1852); "Description of an early human Embryo of about the fourth week of Utero-Gestation" ('Mic. Soc. Trans.,' 5, 1857).

In the 'Medical Gazette' for 1835 (vol. 17), Dr. Farre published a paper on the 'Trichina Spiralis.' In the previous volume of the Gazette the discovery of the parasite is referred to as follows:—"The muscles of bodies dissected at St. Bartholomew's Hospital had been more than once noticed by Mr. Wormald, the demonstrator, to be beset with minute whitish specks, and their appearance having been again remarked in the body of an Italian aged 45, by Mr. Paget, a student of the hospital (now Sir James Paget), who suspected it to be produced by minute entozoa, the suspicion was found to be correct, and Mr. Owen was furnished with portions of the muscles on which he had made the following observations." An account is then given of the observations of Professor Owen, who named the parasite *Trichina spiralis*. A second body infested by this parasite had been observed in the dissecting room of St. Bartholomew's, within a fortnight after the occurrence of the first, and the object of Dr. Farre's paper was, as he said, mainly to refer to some points on which he was able to give some additional information, or as to which his observations differed from those of Professor Owen. In particular he had been able to make out in some specimens the existence of a distinct alimentary canal and an ovary. A paper by Dr. Farre "On *Diplosoma crenata*, an entozoon inhabiting the human bladder, and hitherto often confounded with *Spiroptera hominis*," was published in Beale's 'Archives of Medicine,' vol. 1, p. 290).

The article "Worms," in the 'Library of Medicine' (vol. 5, p. 241), was contributed by Dr. Farre.

At a time when Dr. Farre was overworked and harassed by his large practice, he had the great sorrow of losing his wife, and shortly afterwards he sustained a compound dislocation of the ankle, in consequence of a fall from a second floor window into the area of his house. The wound gradually healed, and he was able to move about, first with crutches, and then with the help of a stick; but he remained painfully lame until his death, which occurred on the 17th December, 1887.

Dr. Farre's lectures and his clinical teaching were highly appreciated by his pupils, of whom the writer of this notice had the good fortune to be one. He was a model physician accoucheur, and he acquired the confidence and esteem of the profession through his diagnostic and practical skill, and the high principles and sense of

honour by which he was guided in all the relations of life. A most charming and genial companion, an affectionate and constant friend, he delighted those who enjoyed the privilege of his society and his generous hospitality by his musical accomplishments, which were of a very high order.

G. J.

GUSTAV ROBERT KIRCHHOFF was born on March 12, 1824, at Königsberg. He began his studies in his native town under the direction of F. E. Neumann, and no one who has studied the writings of both these eminent men can fail to notice the great influence which Neumann's teaching must have had in forming the character of Kirchhoff's scientific ideas.

In 1850 Kirchhoff went as Professor Extraordinarius to Breslau, and in 1854 as Professor of Physics to Heidelberg, where he stayed till 1875. In that year he accepted a chair of Physics in Berlin. Gradually failing in health he had to give up his lectures, and died on October 17, 1887.

His writings, the first of which he published at the age of twenty-one, cover nearly the whole range of physics, and there is hardly one of them which has not marked a decided progress in the subject to which it refers.

His first paper (1845), treating of plane current sheets, was the first of a series in which he deduced and applied the now well-known equations for the distribution of electric currents in conductors which are not linear. In 1849 an important communication appeared in Poggendorff's 'Annalen,' in which, for the first time, the resistance of a wire was measured in what is now known as electromagnetic measure. A paper of considerable interest, "Ueber die Bewegung der Elektricität in Leitern," appeared in the year 1857. The propagation of electric effects in wires is discussed in this paper, the principal result being: "that the rate of propagation of electric waves is found to be $c/\sqrt{2}$—that is, independent of the cross section, the coefficient of conductivity of the wire, and the electric density; the rate is 41,950 (German) miles per second, or very nearly the same as that of the propagation of light." In view of the important conclusions to which modern researches in electricity have led, considerable historical interest will always attach to the above statement. The remaining electrical papers treat of the oscillating discharge of the Leyden jar, the distribution of electricity on two conducting spheres, and of the capacity of a condenser formed of two parallel circular plates.

We have next two papers on Magnetism; one (1853) treats of the magnetism induced in an infinitely long cylinder, and the other solves the problem of the magnetisation of an iron ring under the influence

of electric currents. A suggestion made in the latter paper to use closed rings of iron for the determination of the coefficient of induction has led, at the hands of Stoletow and Rowland, to important practical results.

Kirchhoff's name is most generally known in connexion with his researches on the relation between the absorptive and the emissive properties of bodies. The explanation of the Fraunhofer lines which are derived from these researches, and the work done jointly with Bunsen on the discontinuous spectra of gaseous bodies, gave such an impulse to the study of radiation that a whole science—that of spectrum analysis—developed as a historical sequence to Kirchhoff's work. It is a curious instance of an abstruse calculation giving rise to extended experimental investigations which have in reality very little connexion with it; for it is only a small fraction of spectrum analysis in which the connexion between radiation and absorption is made use of at all. There is really no *à priori* reason why we could not have known as much as we do now of the spectra of different bodies without being acquainted with the important law proved by Kirchhoff.

A discussion has arisen as to how far Kirchhoff's work was anticipated by that of Balfour Stewart. The latter had experimented on the radiation and absorption of heat, and had drawn some important conclusions from his experiments. Stewart's work is conclusive in showing that if we assume the ratio of the emissive to the absorptive power to be the same for all bodies and only a function of the temperature and wave-length, all facts can be satisfactorily accounted for. Kirchhoff, without being acquainted with Stewart's researches, went further, and proved that the law just stated is the only one consistent with thermodynamical equilibrium. Kirchhoff's paper has been objected to as being too elaborate in the method of its proof, but no simpler proof has ever been given, and it would be difficult to lay a finger on a single sentence of this classical paper which could be removed or shortened without detriment to the logical sequence of the argument.

In connexion with these theoretical researches and in order to trace the existence of terrestrial elements in the sun, Kirchhoff prepared a drawing of the solar spectrum. Unfortunately an arbitrary scale was used, and the prisms were occasionally shifted, so that the map was soon superseded by Ångström's, in which the lines were directly referred to wave-lengths. It seems of interest, however, to point out as a proof of the acuteness of Kirchhoff's observing power and the perfection of the optical adjustments, that the amount of detail given in his map is exactly that given by calculation as possible with the resolving power which he used. No work could be more trying than that of drawing a map of the solar spectrum reaching the

limits of the instrumental powers; Kirchhoff's eyes suffered in consequence, and he had to leave the completion of the map to K. Hofmann.

Soon after the mathematical development by Clausius and Sir William Thomson of the mechanical theory of heat, Kirchhoff was the first to carry the application of that fruitful theory into the domain of Chemical Physics. The thermal phenomena connected with the absorption of gases and the dissolution of salts in liquids as well as their relationship with the vapour-pressures of the solvent, were treated in two papers of great interest and importance.

A few words must be said on Kirchhoff's papers on elasticity.

There are few problems which have occupied so many eminent mathematicians, and our author's investigations contributed very materially to the progress of that important branch of theoretical physics. Before Kirchhoff's time Sophia Germain had made an attempt which was only partially successful to establish the equations which regulate the vibrations of thin plates. Poisson had gone a good deal further, especially as regards the treatment of rectangular plates. Kirchhoff points out that Poisson's boundary conditions cannot in general be satisfied, and deduces from the general theory of elasticity a solution which can be applied to circular plates. With the help of measurements made by M. Strehlke, the theoretical results were checked and verified by experiment. In some later papers the elastic deformations of rods were treated in a more general way than had previously been done, and especially in 1879 the solution of the problem was extended to prismatic and conical rods.

The science of hydrodynamics also is indebted to Kirchhoff for several beautiful investigations, amongst which special attention may be drawn to the paper "Zur Theorie freier Flüssigkeitsstrahlen," and to one in which it is shown that two rigid rings in a fluid moving irrotationally exert apparent forces on each other which are identical with those which the rings would show if electric currents were to circulate round them.

Kirchhoff's papers have been collected into a volume of moderate size. He has also published a series of lectures on mechanics, in which we are especially struck with the precision with which the subject is treated, and with the way all metaphysical difficulties in the first definitions are avoided. "For this reason," he says in the introduction, "I take it to be the object of mechanics to *describe* the phenomena of nature, to describe them completely and in the simplest manner. I mean that it will be our task to state what the phenomena are, but not to find out the *causes*." None of those who have attended Kirchhoff's lectures on mathematical physics are ever likely to forget them. Each lecture was complete in itself, and the student felt on leaving the room that he had learnt

something which it would be difficult or impossible for him to find in the published books. He was in consequence a popular and successful teacher, and nearly all the younger German physicists are his pupils.

In Kirchhoff science has lost a man who combined, to an exceptional degree, mathematical talent with observational skill and experimental knowledge.

A. S.

Dr. Balfour Stewart was born in Edinburgh on November 1st, 1828, and died in Ireland on December 18th, 1887, having just entered his sixtieth year. He was educated for a mercantile profession, and in fact spent some time in Leith, and afterwards in Australia, as a man of business. But the bent of his mind towards physical science was so strong that he resumed his studies in Edinburgh University, and soon became assistant to Professor J. D. Forbes, of whose class he had been a distinguished member. This association with one of the ablest experimenters of the day seems to have had much influence on his career; for Forbes's researches (other than his Glacier work) were mainly in the department of Heat, Meteorology, and Terrestrial Magnetism, and it was to these subjects that Stewart devoted the greater part of his life. In the classes of Professor Kelland, Stewart had a brilliant career; and gave evidence that he might have become a mathematician, had he not confined himself almost exclusively to experimental science.

In 1858, while he was still with Forbes, Stewart completed the first set of his investigations on Radiant Heat, and arrived at a remarkable extension of Prévost's "Law of Exchanges." His paper (which was published in the 'Transactions of the Royal Society of Edinburgh') contained the greatest step which had been taken in the subject since the early days of Melloni and Forbes. The fact that radiation is not a mere surface phenomenon, but takes place like absorption throughout the interior of bodies, was seen to be an immediate consequence of the new mode in which Stewart viewed the subject. Stewart's reasoning is, throughout, of an extremely simple character, and is based entirely upon the assumption (taken as an experimentally ascertained fact) that in an enclosure, impervious to heat and containing no source of heat, not only will the contents acquire the same temperature, but the radiation at all points and in all directions will ultimately become the same, in character and in intensity alike. It follows that the radiation is, throughout, that of a black body at the temperature of the enclosure. From this, by the simplest reasoning, it follows that the radiating and absorbing powers of any substance must be exactly proportional to one another (equal, in fact, if measured in proper units), not merely for the radiation as

a whole, but for every definitely specified constituent of it. In Stewart's paper (as in those of the majority of young authors) there was a great deal of redundant matter, intended to show that his new views were compatible with all that had been previously known, and in consequence his work has been somewhat lightly spoken of, even by some competent judges. These allow that he succeeded in showing that equality of radiation and absorption is consistent with all that was known; but they refuse to acknowledge that he had proved it to be necessarily true. To such we would recommend a perusal of Stewart's article in the 'Philosophical Magazine' (vol. 25, 1863, p. 354), where they will find his own views about the meaning of his own paper. The only well-founded objection which has been raised to Stewart's proof applies equally to all proofs which have since been given, viz., in none of them is provision made for the peculiar phenomena of fluorescence and phosphorescence.

The subject of radiation, and connected properties of the luminiferous medium, occupied Stewart's mind at intervals to the very end of his life, and led to a number of observations and experiments, most of which have been laid before the Royal Society. Such are the "Observations with a Rigid Spectroscope," and those on the "Heating of a Disk by rapid Rotation *in vacuo*," in which the present writer took part. Other allied speculations are on the connexion between "Solar Spots and Planetary Configurations," and on "Thermal Equilibrium in an Enclosure containing Matter in Visible Motion."

From 1859 to 1870 Stewart occupied, with distinguished success, the post of Director of the Kew Observatory. Thence he was transferred to Manchester as Professor of Physics in the Owens College, in which capacity he remained till his death. His main subject for many years was Terrestrial Magnetism; and on it he wrote an excellent article for the recent edition of the 'Encyclopædia Britannica.' A very complete summary of his work on this subject has been given by Schuster in the 'Manchester Memoirs' (4th series, vol. 1, 1888). In the same article will be found a complete list of Stewart's papers.

Among the separate works published by Stewart, his 'Treatise on Heat,' which has already reached its fifth edition, must be specially mentioned. It is an excellent introduction to the subject, though written much more from the experimental than from the theoretical point of view. In the discussion of radiation, however, which is given at considerable length, a great deal of theoretical matter of a highly original character is introduced.

Of another work, in which Stewart took a great part, 'The Unseen Universe," the writer cannot speak at length. It has

passed through many editions, and has experienced every variety of reception—from hearty welcome and approval in some quarters to the extremes of fierce denunciation, or of lofty scorn, in others. Whatever its merits or demerits it has undoubtedly been successful in one of its main objects, viz., in showing how baseless is the common statement that "Science is incompatible with Religion." It calls attention to the simple fact, ignored by too many professed instructors of the public, that human science has its limits; and that there are realities with which it is altogether incompetent to deal.

Personally, Stewart was one of the most loveable of men, modest and unassuming, but full of the most weird and grotesque ideas. His conversation could not fail to set one a-thinking, and in that respect he was singularly like Clerk-Maxwell. In 1870 he met with a frightful railway accident, from the effects of which he never fully recovered. He passed in a few months from the vigorous activity of the prime of life to grey-headed old age. But his characteristic patience was unruffled and his intellect unimpaired.

He became a Fellow of the Royal Society in 1862, and in 1868 he received the Rumford Medal.

His life was an active and highly useful one; and his work, whether it took the form of original investigation, of accurate and laborious observation, or of practical teaching, was always heartily and conscientiously carried out. When a statement such as this can be truthfully made, it needs no amplification.

P. G. T.

Dr. Owen Rees was born at Smyrna in November, 1813. His father was a Levantine merchant, and married an Italian lady, by whom he had a large family. Owing to his father's failure in business, he was obliged to be educated at a private school, and for the same reason many of his family in after years were compelled to reside with him. This is probably the explanation of his remaining unmarried. He, however, found a good patron in his uncle, who was a partner in the publishing firm of Longman and Co. In 1829 Owen Rees was apprenticed to Mr. Richard Stocker, the apothecary at Guy's Hospital, and he very soon showed his inclination towards scientific pursuits, and especially to chemistry. He attracted the attention of Dr. Bright, who requested his assistance in the analysis of the secretions in diseases of the kidney, and in this way a lifelong friendship sprang up between them. He made quantitative analyses of the albumen and urea in the urine, and proved the presence of the latter in the blood. His papers on this subject are to be found in the 'Medical Gazette' for the year 1833.

In the year 1837 he took his degree at Glasgow, and shortly afterwards published a small work entitled 'Analysis of the Blood and

Urine in Health and Disease.' In his preface the author says "the increased desire for more intimate acquaintance with animal chemistry, which has lately been evinced by the medical profession, induces me to present this little work to public notice. The more philosophical modes of investigation at present adopted to ascertain the diseased conditions of the living system, have found a new branch of inquiry deserving the attention of the student." He then describes his method of separating the various ingredients of the blood, and as regards his discovery of urea he evaporated the blood to dryness and treated the residue with ether. He also made an exhaustive analysis of the urine, showing that the colouring-matter was a distinct principle, and gave the various tests for albumen. Bright had already observed that the phosphates were precipitated by heat, and thus was sometimes a cause of error in testing for albumen. He referred to Rees for an explanation, who, after a series of experiments, determined that it was muriate of ammonia which kept the phosphates in solution, and that it was the decomposition of this which thus necessarily caused their precipitation. For the presence of sugar he did not use chemical tests, but evaporated the urine and dissolved out by alcohol. In a paper published in 1838 in the 'Guy's Hospital Reports,' Rees showed how sugar could be obtained from diabetic blood, as its presence therein had been previously doubted. He evaporated the blood, soaked the residue in water, treated with ether to remove urea and fat, and then allowed the remainder to crystallise.

In the year 1841 Rees made, in conjunction with Mr. Samuel Lane, some very important observations on the corpuscle of the blood. They concluded that it was a flattened capsule containing a coloured fluid, and they further showed the changes which it underwent on the application of reagents as saline fluids and syrup. They inferred from this that in the living body there must be a similar osmotic change going on, as in anæmia and in cases where the saline substances were in excess. Rees subsequently made observations on the nucleus of the corpuscle in different animals, and showed the similarity of the white corpuscle to that of lymph and pus. Although finding urea in various secretions in cases of diseases of the kidney, yet often failing to discover it in the blood, he doubted whether its presence was the cause of convulsions and other nervous symptoms so frequently met with in *Morbus Brightii*.

He communicated two papers to the Royal Society—one in 1842 entitled "On the Chemical Analysis of the Contents of the Thoracic Duct in the Human Subject." The fluid was obtained from a criminal executed at Newgate, and taken an hour after death. An article on chyle and milk, written afterwards, is to be found in Todd and Bowman's 'Cyclopædia of Anatomy and Physiology.' His other paper presented to the Royal Society was entitled "On the Formation

of the Red Corpuscles of the Blood and on the Process of Arterialisation." His theory was that the venous blood contained phosphorus in combination with fat, and that an oxidation took place, and the phosphoric acid united with the alkali of the blood, producing a tribasic phosphate of soda. This caused the bright colour of arterial blood.

Rees also wrote a little book on calculous diseases. This consisted mainly of the Croonian Lectures given at the College of Physicians in the year 1856. In the 'Guy's Hospital Reports' various papers will be found on chemical analysis of animal fluids. Owen Rees, in many instances, joined Dr. Alfred Taylor in his criminal investigations, notably in the case of Palmer, who was tried for the murder of Cook by strychnia in the year 1856. He also assisted Sir B. Brodie in his analysis of urinary calculi, and it was owing to this surgeon's influence that Dr. Rees gained the appointment of Physician to the new Pentonville Prison.

Dr. Owen Rees was appointed Assistant-Physician at Guy's Hospital in the year 1843, and full Physician in 1856. In 1873 he resigned, and was appointed Consulting Physician. He was a particular friend of Dr. Roget, who presented the papers spoken of to the Royal Society, of which Dr. Roget was then Secretary. It was soon afterwards, in the year 1843, that he was made a Fellow.

Dr. Rees was in practice first in Cork Street and afterwards in Albemarle Street. His clients were amongst the better classes, and usually sufferers from kidney disease or gout, for the treatment of which disorders he had gained considerable repute. Personally, he was a small, lithe, active man, ready and sociable, so that he was a well-known member at the Athenæum and many convivial clubs. He was very quaint and humorous in his stories, so that his company was much sought after. He was, in his latter days, made Physician Extraordinary to the Queen. In the beginning of 1886 he was seized with a paralytic stroke, but although he partly recovered he never did much work afterwards. On May 27th, 1889, he had another seizure, which proved fatal, and he was buried in Abney Park Cemetery, aged 76 years.
S. W.

Sir Charles James Fox Bunbury, Bart., was born at Messina, in Sicily, February 4th, 1809, where his father, General Sir Henry Bunbury, was at that time Quartermaster-General. His mother was a daughter of General Fox, then commanding in the Mediterranean, and a niece of the celebrated statesman. To these gifted parents Sir Charles owed his early love and his knowledge of arts, literature, and science, and especially of natural history, accomplishments which he cultivated throughout life with disinterested zeal; and thanks to his extraordinary memory, his accuracy was as remarkable as were the extent and variety of his information.

After completing his education at Trinity College, Cambridge, Mr. Bunbury visited Brasil and the River Plate, whither he was attracted by the fact of his uncle, Mr. Fox, himself an ardent collector of plants, being Minister at Monte Video. This was followed by a voyage to South Africa, where another uncle, General Sir George Napier, was Governor of the Cape Colony; and in 1853 he accompanied Sir Charles Lyell to Madeira and Teneriffe. In all these countries Sir Charles Bunbury made extended excursions, observing diligently and collecting assiduously, though travelling as an amateur rather than a scientific naturalist. The results of these journeys are full of interest to the botanist, zoologist, and geologist; they are published in various scientific periodicals, and in a 'Visit to the Cape,' which appeared in 1847. Especially valuable are the botanical observations made in South Africa and South America, which deal with the broad features of a vegetation known previously only in detail. They are brought together in a volume published shortly before his death, entitled 'Botanical Fragments.'

It is, however, by his researches in vegetable palæontology that Sir Charles Bunbury is best known as a scientific man. To this subject his attention was more immediately drawn through his connexion by marriage with Sir Charles Lyell, and his most valuable contributions to it may be said to be ancillary to Sir Charles's investigations into the coal-measures of British North America and the United States; they appeared in the form of a succession of communications to the Geological Society of London between 1846 and 1861, and are printed in that Society's Journal. He also wrote on the Carboniferous flora of the Tarentaise, on the Anthracites of Savoy, on the Jurassic flora of Yorkshire, on the Fossil plants of Nagpur in the Deccan Peninsula, and of the Island of Madeira. Under this head, too, should be recorded the great services he rendered to palæontology, by classifying and naming the Carboniferous fossils in the Museum of the Geological Society (of which Society he was Foreign Secretary from 1847 to 1853). This collection was for many years the only one of its kind in England available to geologists or botanists.

In 1844 Mr. Bunbury married Frances Joanna, daughter of Leonard Horner, Esq., F.R.S., and sister to Lady Lyell, who survived him. In 1860 he succeeded, through the death of his father, to the baronetage, and removed from the Manor House of Mildenhall, to the family seat, Barton Hall, Bury St. Edmunds, where he died June 18th, 1886, leaving no descendants. He was a Fellow of the Linnean and Geological Societies, as well as of the Royal Society, into which he was elected in 1851.

J. D. H.

Asa Gray was born November 8, 1810, in the township of Paris, Oneida Co., New York. He was descended on the father's side from a Scotch-Irish family that had emigrated early in the 18th century. His mother was a lady of English descent. As a boy he assisted in his father's farming, and, showing an aptitude for and love of book-work, he was sent, first to a private school, thence to the Grammar School of Clinton, N.Y., and lastly to the Fairfield Academy. To this latter institution a medical school of repute was attached, and it was through attendance on its lectures that Gray became interested in scientific pursuits, and especially in botany, through an article in 'The Edinburgh Encyclopædia.' In 1825 he entered the Fairfield Medical School, the sessions of which occupying only half the year, left him ample time for following his favourite pursuits of botany and mineralogy in the study and in the fields. In 1831, having taken his doctor's degree at Fairfield, he forthwith abandoned the thoughts of medicine as a profession, and accepted the post of Instructor in Chemistry, Mineralogy, and Botany in the High School of Utica, N.Y. After spending several years in botanising and in lecturing in various educational establishments, he became Assistant to Professor Torrey in the Chemical Laboratory of the Medical School at New York. This, though it led to nothing professionally, was perhaps the turning point in Gray's career, for Torrey was an able and ardent botanist, who had already recognised Gray's ability, and a life-long friendship was established between them, to be emphasised by the joint publication of 'The Flora of the North American Continent,' a work justly esteemed as second alone to De Candolle's 'Prodromus Regni Vegetabilis' as a contribution to a knowledge of the vegetation of the globe.

In 1835 Gray became Curator and Librarian of the New York Lyceum of Natural History, a post which gave him abundant leisure; and this he employed in the preparation of his 'Elements of Botany,' which was at once accepted as the best text-book of the science that had appeared in the States, and as second to none in the English language. The 'Elements' is the first of a series of educational works on morphological, physiological, and systematic botany that have been for half a century the class-books of schools and colleges throughout the United States and in British America, and which have been cordially recommended by teachers in England as the best of their class. Nor should mention be omitted of two smaller educational works, entitled 'How Plants Grow,' and 'How Plants Behave,' which, "for the interest of their subject, the elegance of their diction, and the lucidity of their style, have led the general public to appreciate the scientific aspect of botany more perhaps than have any other in the English language."*

* Of these *libelli* a competent American author has written that "they found

In 1836 Dr. Gray was appointed Botanist to Commodore Wilkes's voyage of exploration in the Pacific and Antarctic regions; but after experiencing innumerable delays, and uncertainties as to the management of the expedition, and as to his own position in it, he withdrew from the enterprise. It is idle to speculate on the loss to science thus incurred. It may reasonably be assumed, however, that if science lost much in the form of invaluable observations, original researches, and copious collections, America gained much by the retention of Gray's personal influence, his teaching, and the elaboration of its flora.

Settling in New York, Gray now devoted his whole energies in conjunction with Torrey to 'The Flora of North America,' of which the first parts appeared in 1838, and the last that have been published very shortly before his decease. Torrey was the projector of the work, but even from the first the lion's share fell to Gray, who is practically its sole author. To put briefly the amount of labour involved in Gray's systematic and descriptive publications on the North American Flora, it may be stated that they may embrace in one form or another a great proportion of the 10,000 or 11,000 species that the continent possesses. Of these more than half are so carefully and methodically described in the volumes of the Flora that have appeared, that there is perhaps no instance of a species being misplaced as to genus, and in few could a better grouping of species into subordinate divisions be suggested. The remainder are described in more or less detail in innumerable papers, in answer to demands for immediate publication in the 'Reports of Government Expeditions,' or in the works of travellers and collectors. These scattered publications would, as it was hoped by the author, have accelerated the pace of the great work; but the contrary was the result, and the prospects of the completion of the Flora are far distant, if existent.

Of Gray's numerous other works four are especially noteworthy: his 'Manual of the Botany of the Northern United States,' of which it is justly written by an eminent American author, that the botanist has yet to be born who could give a more clear, accurate, and compact account of the flora of a country; the 'Genera Floræ Americæ Borealis Orientalis illustrata,' a work designed to illustrate and describe the morphology, affinities, and distribution of plants in the area indicated, but discontinued after the first two volumes; the 'Botany of Commodore Wilkes's South Pacific Exploring Expedition,' of which a quarto volume, with a superb atlas of plates, was published, when the withdrawal of funds arrested its further progress; lastly, an essay on the flora of Japan, which, in point of

their way where botany as botany could not have gained an entrance, and they set in motion a current which moved in the general direction of a higher science with a force that can hardly be estimated."

originality and far-reaching results, is the author's *opus magnum*. In it, by a comparison of the floras of Eastern and Western America with one another and with Japan, and of all with the Tertiary flora of North America, Gray has outlined the history of the vegetation of the north temperate zone in relation to its past and present geographical features, from the Cretaceous period to the present time. The above are works of research, but there remain two labours of this most industrious botanist which demand a notice; these are the part he took in the controversy that followed the publication of Darwin's 'Origin of Species,' and his extraordinary activity as a reviewer, bibliographer, and historian of the progress of botany during his life-time.

Gray was one of the first to accept and defend the doctrine of natural selection, which he further fortified by masterly reasoning, judicious criticism, and by experiments; so that Darwin, whilst fully recognising the different standpoints from which he and Gray took their departures, and their divergence of opinion on important points, nevertheless regarded him as the naturalist who had most thoroughly gauged the 'Origin of Species,' and as a tower of strength to himself and his cause. It is not needful to dwell further on this subject; Gray's intimacy with Darwin, and their most interesting and instructive correspondence, are matters of history, and together with their divergent sentiments, are fully set forth in the life of the latter, and in two works by Gray entitled 'Darwiniana' and 'Science and Religion.'

It remains to allude to Gray's labours as a reviewer and bibliographer. Amongst his many accomplishments, not the least was his intimate knowledge of the early and latter history of botany, and of the writings and doings of botanists everywhere. For fifty years (the last thirty as co-editor of 'Silliman's Journal') he kept the American scientific public fully informed of the progress of his favourite science in Europe and elsewhere, of all publications of value, of the movements of botanical travellers, and of the changes in the staff of museums, gardens, herbaria, &c. An American* colleague says of him, "As a reviewer he was certainly extraordinary. Some of his reviews were, in reality, elaborate essays, in which, taking the work of another as a text, he presented his own views on important points in a masterly manner. Others were technically critical, while some were concise and clear summaries of lengthy works. Taken collectively, they show better than any other of his writings the literary excellence of his style, as well as his great fertility and acuteness as a critic. Never unfair, never ill natured, his sharp criticism, like the surgeon's knife, aimed not to wound, but to cure; and if he sometimes felt it his duty to be severe, he never failed to praise what was

* Professor Farlow, "Memorial of Asa Gray," in the 'Proceedings of the American Academy of Arts and Sciences,' p. 33.

worthy. His style was so easy, so flowing, and so constantly enlivened by sprightly allusions and pleasing metaphors, that one can read what he wrote for the mere pleasure of reading it. He was one of the rare cases where science had appropriated to herself one who who would have been an ornament to any purely literary profession."

In 1842 Gray was appointed to fill the newly-endowed chair (the Fisher) of Natural History in Harvard College, to which was attached the direction of the Botanical Gardens at Cambridge, Mass. From this time he devoted his chief energies to creating a botanical library and herbarium, and to the continuation of the Flora of North America. For upwards of thirty years he fulfilled the duties of Lecturer and of Director of the Gardens, during which he raised the whole botanical establishment, garden, library, and herbarium, to first-rate importance. In 1872 he was relieved of the duties of lecturing, and shortly after of the charge of the garden, and had an assistant appointed to the herbarium; but he retained the title of Fisher Professor and Director of the Gardens until his death.

Having made six visits to Europe, entailing several lengthy stays in England, in furtherance of the Flora, his wiry figure, his vivacity, and the alertness of his intellect were well known in this country, where his highly cultivated mind and the charm of his personality won him friends in all circles of society. His last visit was in 1887, soon after his return from which he was struck with paralysis, and died on the 30th of the following January, on his seventy-eighth birthday.

Dr. Gray married, in 1858, Jane, daughter of C. G. Loring, of Boston, and had no family. He was a correspondent of the Institute of France, a Doctor of Laws of Oxford and Edinburgh, and a Doctor of Science of Cambridge. He was elected foreign member of the Linnean Society in 1850, and of the Royal Society in 1873. He was a fellow or correspondent of the principal Continental scientific academies, and had served as President and for sixteen years as Corresponding Secretary of the American Academy of Arts and Sciences, and of the American Association for the Advancement of Science, and he was a Regent of the Smithsonian Institution. He is credited down to the year 1873 with no fewer than 107 papers in this Society's Catalogue of Scientific Papers, and with upwards of 350 scientific works and papers in a chronologically arranged catalogue of these, appended to the 36th volume of the 'American Journal of Arts and Sciences' (September, 1888).

J. D. H.

Sir William O'Shaughnessy Brooke, F.R.S., died on the 8th of January, 1889, at Southsea, after a short illness. He was born in Limerick in 1809, and was therefore in his 80th year. His original

name was William O'Shaughnessy, but he took the name of Brooke on the death of a relative of that name. He graduated as M.D. at Edinburgh in 1833, and shortly afterwards joined the Bengal Army as Assistant Surgeon. He was promoted to Surgeon in 1848, and to Surgeon-Major in 1858. In 1835 he was appointed Professor of Chemistry in the Medical College at Calcutta. From 1844 to 1851 he acted as Assay Master of the Calcutta Mint. He was of a scientific turn of mind, and took readily to the experiments made by Cooke and Wheatstone in telegraphy in 1837. He made some experiments in submarine telegraphy across the Hooghly, at Calcutta, in 1839, and he was the first to introduce the telegraph into India. In 1852 he was appointed Superintendent-General of Telegraphs in India, and he retained that post until 1862, when he resigned and retired from the Indian Medical Service. He was the author of several papers on scientific and engineering subjects, many of which appeared in the Journals of the Asiatic Society. He was knighted for his valuable services in establishing the service of telegraphs throughout India, and he was elected a Fellow of the Royal Society in 1843.

W. H. P.

Walter Weldon, F.R.S.; born 31st October, 1832; died 20th September, 1885. Very little is known of the early history of the subject of this memoir, beyond the bare facts that he was born at Loughborough on the 31st October, 1832; that he was the eldest son of a manufacturer in that town, and was employed for some years in his father's business; that while so occupied he discovered a taste for literature; and that, in his twenty-second year, he left his native town, and arrived in London determined to make his way as a journalist. With him he brought his young wife, whom he had just married, and who was destined to be his never failing source of consolation and encouragement during those early years of difficulty and adversity which are invariably experienced by a young man beginning an independent life under such circumstances. Young Weldon most certainly had his full share of dark days; and in speaking of them in after life to those who knew him well, he loved to dwell on the sustainment and encouragement he had derived from his devoted wife. Apart from this, however, he was endowed by Nature with a variety of gifts, any one of which might have enabled him to acquit himself well in the battle of life, the combination of which was irresistible, and served not only to raise him to a position of distinction and honour, but to render him capable of performing work which has undoubtedly been, and will probably long continue to be, a benefit to civilised humanity.

Mr. Weldon's first journalistic work was done in connexion with

'The Dial,' afterwards incorporated with 'The Morning Star,' now dead, but in its time a Liberal daily paper of some importance. While thus engaged, he conceived the idea of issuing, at a price which should bring it within the reach of all, a monthly journal devoted to recording current progress in literature and science—subjects which were ever twin sisters in his mind. The connexions he had as a journalist made among writers and scientists on the one hand, and country booksellers on the other, put the production and issue of such a work within his power; and that there was likely to be a demand for such information as he sought to give he felt convinced from his own youthful needs and experiences. His purpose was that the periodical should be issued in London under his own name, and simultaneously in various provincial towns, in each case as the journal of any local bookseller who should subscribe for a certain number of copies. Accordingly, the first number of 'Weldon's Register of Facts and Occurrences in Literature, Science, and Art' was published in London on the 1st August, 1860, at the price—moderate, even when compared with the cost of such periodicals at the present time—of 6*d*. Its proprietor spared no effort to render the work valuable and attractive to those who sought its pages, and secured for it contributions from many men then or since distinguished in science, letters, or art. But those were not the days of an earnest popular desire for such information as 'Weldon's Register' sought to impart, and, without such a demand, at so low a price, it had no chance of success. Consequently, after an existence of some three years, it was abandoned by its projector, who now turned his attention to another and strangely different object.

The practical failure of an ardently cherished scheme, the abandonment of work for which he rightly considered himself specially fitted, was no doubt a severe blow. But Mr. Weldon knew not failure in the ordinary sense. To come short of success in one way was, with him, but an incentive to seek it with redoubled vigour in another. No one could know him well and observe him closely without recognising not only the great scope of his talents, but also the marvellous avidity and thoroughness with which he grasped any subject upon which he brought his mind to bear. In him, moreover, a strong and active mind was allied with constitutional power and a capacity and love for work which it is the good fortune of but few to possess. This is the simple and only explanation of the astonishing fact that, without any education in science, without any technical training, without the advantage to be gained by attending lectures or watching the performance of experiments—with nothing to help him through but his genius and the knowledge he could acquire by sheer hard reading at the British Museum—Mr. Weldon now attacked the problems and difficulties of industrial chemistry. Why he turned

his attention to a task so different from all his previous occupations cannot be stated with certainty. In an admirable memoir of him, published in the 'Journal of the Society of Chemical Industry' in October, 1885, it has been suggested that he derived his first impulse to this new work from his friend the late Charles Townsend Hook, the well-known paper maker of Snodland; and the writer has good reason to believe that the suggestion is well grounded. From whatever cause, Mr. Weldon, from that time to the very hour of his death, devoted himself, with all the earnestness, zeal, and energy of which he was capable, to the study of industrial chemistry, and of the chlorine and alkali manufactures in particular. He thoroughly mastered his subject in all its details, whether from a scientific or a business point of view, insomuch that during the last ten years of his life he was regarded as the highest authority upon it, not only in England, but wherever the manufacture of alkali was carried on. It may safely be averred that, so long as the practice or the memory of this industry shall last, so long will the name of Walter Weldon be honourably associated with it.

The great work of his life, his process for the perpetual regeneration of the manganese oxide used in the production of chlorine from the hydrochloric acid which is the by-product of the Leblanc soda process has been so often described, and is so widely known, as to need no detailed explanation here. No better description of it exists than that, written by the hand of the inventor, published in the 'Journal of the Society of Chemical Industry,' Sept., 1885 (vol. 4, p. 525). Suffice it to say that it produced a revolution in the industry which it affected, supplanting a method of working at once crude, wasteful, and noxious; that it has saved an expenditure to this country of about £750,000 per annum since it has been in operation; and that it has been one of the chief factors in bringing the purchase of bleached fabrics—linen, calico, paper, &c.—well within the power of the poorest classes. To *invent* a good and workable chemical process, however, is not sufficient; and it was perhaps as an *exploitant* and as a man of business (though without a trace of the sordid attributes of the mere *business man*), in the power of fixing the interest, influencing the minds, and attracting the sympathies of those with whom he had to deal, that Mr. Weldon specially excelled. But for these powers, it is doubtful whether his process—so advantageous in its use, and so facile in its working—would ever have been put into practical execution. With all these advantages, it required many years of arduous and anxious work—not to mention the taking of many patents—before Weldon's process became, what it still is, the almost universally adopted method of producing chlorine.

Patented in 1866, and temporarily experimented with soon after-

wards at the works of the Walker Alkali Company, on the Tyne, it was not until late in 1868 that Weldon's regenerated-manganese chlorine process was in operation on a manufacturing scale, at the works of Messrs. J. C. Gamble and Sons, at St. Helens. By the end of 1870, however, out of about forty chlorine makers in the United Kingdom, thirty-five had taken licences for its use, seventeen plants being then either actually at work or on the point of completion. But a formidable rival now arose, in the shape of Mr. Deacon's process for producing chlorine by passing hydrochloric acid gas over masses of pumice stone heated to a certain temperature and saturated with cupric sulphate. Questions of cost apart, the chief merits of this process were (1) its great simplicity; (2) that, per unit of NaCl, it yielded a larger proportion of chlorine than did the Weldon lime process. To meet it, Mr. Weldon again attempted to work the process which he had patented contemporaneously with his lime process, for treating manganese chloride residues by magnesia, so as to recover in the form of hydrochloric acid the proportion of chlorine otherwise lost as calcium chloride. But the Weldon lime process withstood the attacks of all competitors, and by the end of 1875 it had been adopted by every chlorine manufacturer of importance throughout Europe. From the time of the introduction of his chlorine process until the end of 1881, when the patents for it were about to expire, and when his active and pecuniary interest in it ceased perforce, Mr. Weldon was constantly occupied in travel and work connected with the setting up of installations in all parts of the United Kingdom and the Continent, and in devising minor improvements in working. The spring of 1882 brought him relief from this kind of work, but no leisure. Moreover, he had recently suffered the severest bereavement that can befall a man: the sudden death of his younger son, Dante, had been followed only a few weeks later by that of one of the best wives that ever graced an English home. He now, too, began to feel severely the effects of that overwork and unsparing exposure of himself in the cause of duty which had been going on for fourteen years, sapping a constitution once of the most robust.

Many men in his position, under the combined influences of success and sorrow, would now have been content to retire more or less from the active work of life: not so Mr. Weldon. Indeed some of the most useful, and possibly the greatest, work of his life was yet to be accomplished. During 1881 he had been conspicuous as one of the founders of the Society of Chemical Industry—a Society already the most useful, the most successful, and, with one exception, the largest of its kind. To the development and successful working of this Society he devoted an infinite amount of loving labour and thought, whether as member of its Council, as Chairman of its London section,

or as President, which last position he filled from July, 1883, to July, 1884. To the meetings of this Society he contributed many important papers, all bearing on the industry which he had made his special study, and some of them—notably that "On the Present Position of the Soda Industry" (8th January, 1883); his Presidential Address (9th July, 1884); and that on "The Proposal to Raise a Memorial to Nicolas Leblanc" (5th March, 1885)—of such literary as well as intrinsic merit as to create a deep and lasting impression on those who heard or read them.

It may truly be said of Mr. Weldon that he was always working and always learning. Every hour spent in travel, every hour that could be snatched from actual pen-work (for, until the last three years of his life, he did nearly the whole of the vast correspondence incidental to his position with his own hand, often spending some ten hours a day in sheer writing) was devoted to the planning or studying out of new chemical processes or improvements on old methods. From the earliest years of his chemical work, as has been shown above, he had cherished and laboured at the idea of a cheap and practicable magnesia-chlorine process; and he was now to have the satisfaction of arriving at a solution of the problem from a chemical point of view, and also of seeing the mechanical difficulties of such a method in a fair way to be mastered by the engineering skill of his close friend and *collaborateur*, Monsieur Pechiney. This new chlorine process—which its inventor confidently expected to produce as great a revolution in the alkali trade as had resulted from the adoption of his lime-manganese process twenty years ago—was fully described by Mr. Weldon in his memorable address to the Society of Chemical Industry, at Newcastle, on the completion of his Presidential year, in July, 1884; and again in the Journal of that Society for September, 1885. It may be briefly described as follows: Solution of chloride of magnesium—obtained either by the neutralization of hydrochloric acid by magnesia, or by decomposing the residual ammonium chloride of the ammonia-soda process by magnesia; or as the Stassfurt native salt—is evaporated down to a certain point. Sufficient magnesia is then added to produce a solid mass containing about six equivalents of water. This mass is further dried and is then crushed into morsels of about the size of a walnut, which morsels are subjected to a current of air in a special furnace, invented by M. Pechiney, the result being that nearly the whole of the chlorine present is evolved, partly in the free state and partly as hydrochloric acid. The process has been in regular operation at the works of Messrs. A. R. Pechiney et Cie., Salindres, since July, 1887, with highly satisfactory results.

Mr. Weldon was well known as a regular attendant at the meetings of the British Association, and as a frequent contributor of papers to

Section B. Indeed, the last labour of his life, his work 'On the Ratios One to Another of the Atomic Weights of the Elements,' Chapter I of which, "The Glucinum Family," was printed only a few days before the Aberdeen Meeting of 1885—was done with a special view to its consideration by the members of the Association. At what cost *he* went to Aberdeen, in September, 1885, none but the few friends who saw him there can realize. But he had never missed one of the meetings of the British Association since he first attended with Mrs. Weldon in 1865, and nothing could shake his resolve to go. So, though overwhelmed with work and full of bodily pain, he hurried from Germany, and, disregarding the remonstrances of his friends, went straight on to Aberdeen; but only to break down completely on arriving there. After remaining for eight days a prisoner in his hotel, he determined to return alone to his home in Surrey. There he arrived on the 16th September, in such a condition as to inspire the gravest alarm in his friends. Much too late, he now sought that relief in complete rest which he had been entreated to take for years past, doing nothing more during the next three days than dictate a few short letters to old personal friends. In one of them he said: "All work of all kinds is forbidden me—a prohibition which, of course, I shall not be able to obey. But the change which I have so long dreaded is certainly come at last. I had trusted to be able to keep in harness to the very last; but they tell me that a day's work such as I have been accustomed to all my life would be simply suicide, and that of course is not permitted." Late on the afternoon of the 19th, the writer left him in the full belief that he was really better; but the presaged change had indeed come; for at 9 o'clock the next morning he was summoned back to that bedside, only to find that the spirit had already passed away.

For his many and valuable services to chemical industry, Mr. Weldon received the following distinctions: In France, the Great Medal of La Société d'Encouragement, and the Chevaliership of the Legion of Honour. In this country, he was elected a Fellow of the Royal Society (in 1882), Vice-President of the Chemical Society, Vice-President of the Institute of Chemistry, and President of the Society of Chemical Industry.

F. W. R.

THE late SIR JULIUS VON HAAST was born at Bonn, in Germany, on the 1st May, 1824, his father being a wealthy merchant of that city.

After passing through the grammar schools of Bonn and Cologne, the subject of our memoir entered the University of Bonn, and devoted a considerable portion of his time to geological and mineralogical studies. After leaving the University, he spent some years

in France. He afterwards made extensive journeys over many parts of Europe, visiting Russia, Austria, and Italy.

A large firm of shipowners, who wished to direct the stream of emigration from Germany to New Zealand, made him an offer to visit that colony on their behalf, for the purpose of ascertaining its fitness as a field for emigration, an offer which Mr. Haast accepted. Arriving at Auckland in December, 1858, he met Dr. Ferdinand Hochstetter, of the Austrian "Novara" Exploring Expedition, who had just undertaken a geological examination of the North Island on behalf of the Colonial Government. The financial state of the colony at that time, and the disturbances with the Maoris, at once convinced Mr. Haast that it was hardly a suitable field for his countrymen. He accordingly terminated his agreement and joined Dr. Hochstetter's expedition through the North Island. He was subsequently employed by the Provincial Government of Nelson to explore the western and southern portions of that province. With only four companions, he started on an expedition which took him away from civilised life for a period of eight months. During this journey, in addition to the discovery of the Grey and Buller coal fields, and of several gold-bearing districts, he filled in the topography of a large part of Nelson, and added largely to the knowledge of the geology, as well as the fauna and flora, of these alpine portions of New Zealand. On his return, the Government published a full report of the journey, and of the scientific and other discoveries which had been made. This report attracted some attention in Europe, and the Royal University of Tübingen, in 1862, bestowed upon the author the honorary degree of Doctor of Philosophy.

The undoubted success of this Nelson enterprise induced the Canterbury Government to offer Dr. Haast the position of Provincial Geologist. Accepting the offer, he commenced work by similarly investigating the topography and mineral resources of the western ranges of that province. After several years of continuous labour this work was carried to a successful issue, and the practical results were embodied in a voluminous Report on the Geology of the Provinces of Canterbury and Westland. Years before the publication of this complete report the detached accounts of Dr. Haast's explorations were very favourably received and commented on. Sir R. Murchison, the President of the Royal Geographical Society, thus referred to them:—"He was proud to preside upon an occasion when a gentleman, who was a geologist by profession, had proved himself to be a good geographer, and had shown how intimately the subjects of physical geography and geology were united. Dr. Haast's labours were worthy of all commendation." To the general reader the book is a highly interesting and instructive one. The very large amount of geological detail, and the breadth and completeness of the author's

generalisation as to the stratification and the mode of formation of the Southern Alps, their subsequent carving and denudation by ice and water, the evidence of a glacial epoch similar to that which produced the striæ and boulders of Europe, as well as the account given by him of the nature of Canterbury's rivers and the formation of its plains, all testify to the industry and acute observation of this learned *savant*. He was elected a Fellow of the Royal Society in 1867. Some fifty academies and learned societies in various parts of the world enrolled him as a member. The Emperor of Austria conferred upon him a patent of hereditary nobility; and, besides receiving several foreign orders, he was created a C.M.G. by Her Majesty. In 1876 he was appointed Professor of Geology to the Canterbury College, New Zealand University; and in 1880 he was elected a member of the Senate. During his explorations as Provincial Geologist, he commenced the formation of the Canterbury Museum, which, although of such recent growth, has now attained such proportions as to be classified by competent authorities as about the thirteenth in rank of the museums of the world, whilst it undoubtedly is the finest in the southern hemisphere. In 1886, Dr. von Haast came to England as one of the Commissioners for the colony at the Colonial and Indian Exhibition; and for his services on that occasion was promoted by Her Majesty to the rank of K.C.M.G. In 1887 he returned to New Zealand, with his wife and daughter, and died somewhat suddenly of heart disease soon after his arrival at his Canterbury home.

W. L. B.

The task of writing an obituary notice of the late Dr. C. J. B. Williams is rendered comparatively easy by the fact that about four years before his death this venerable physician published full memoirs of his life and work, which have saved much trouble and time in ascertaining the various incidents of his career.

Dr. Williams, born in 1805, was descended from Welsh parents; his father was the Rev. David Williams, Perpetual Curate of the Collegiate Church of Heytesbury, in Wiltshire, and his mother was the daughter of a surgeon, Mr. Williams, who lived at Chepstow. This lady received some instruction from the late Mrs. Hannah More, who, strictly religious as she was, often took her pupils to witness the acting of Garrick, which she considered an important aid in education. His father, engaged at one period of his life in tuition, especially in the preparation of students for the Universities, when he ceased to receive pupils still conducted the education of his sons; but, although Dr. Williams' brothers were afterwards sent to public schools, he himself completed his education at home under his father's superintendence until he entered the University of Edinburgh. Dr.

Williams seems to have made fair progress in classical learning, but never advanced far in this direction, his mind being interested more in the natural sciences; in after years he came to the conclusion that having had the power of directing his mind for some years to the subjects he liked most was in his case productive of much good, and enabled him to develop his power of originality, which might probably have remained for ever latent had he undergone a rigid course of training in any public school during that period of his life. The writer thinks that frequently this has proved true, and that many of our most original discoverers and advancers of knowledge would have failed to attain great eminence had they been obliged for some of the best years of their lives to pursue studies uncongenial to their tastes.

During his stay at home there is one incident in connection with his amusements which may have had some influence over his future acoustic studies; he says he was very fond of birds and animals; he had his pets and used to spend a good deal of time in the poultry yard, and made a special study of the language of cocks, hens, and chickens, ducks and drakes, turkeys and geese, and in short of all domestic animals, and having a nice ear and considerable power of mimicry, he learnt their various notes, and was able to imitate them well enough to influence the creatures towards him as if he had been one of themselves; he remarks that "the brute utterances have all their meanings, and are expressive of various feelings, whether pain or pleasure, anger or love, fear or confidence, defiance or submission, and are mutually intelligible among different animals as words are among human beings."

In the autumn of 1820, Dr. Williams went to Edinburgh as a student of medicine, studied chemistry under Dr. Hope with much pleasure, and anatomy under Professor Monro (*tertius*) and Dr. Barclay with much less satisfaction; he remarks that at that time the teaching of anatomy was very different from what it is at present, from the absence of plates and manuals; there also he studied botany during the summer, and medical jurisprudence under the late Sir R. Christison. At the same time he commenced his attendance at clinical lectures and hospital practice, and became a pupil and great admirer of Dr. Alison, the Professor of the Institutes of Medicine.

In the autumnal vacation, Dr. Williams returned home, the only holiday he had during the period of his medical studies, and in this vacation he was not without amusing resources. He was occasionally gratified by a visit to the theatre, and at different times witnessed the performances of Edmund Kean, Charles Young, Macready, Charles Kemble, the elder Matthews, and of Mrs. Henry Siddons, Miss Stephens, and Miss Paton.

Before leaving Edinburgh, Dr. Williams read a paper at the Royal

Medical Society (1823), afterwards published as a separate thesis, entitled "On the Blood and its Changes by Respiration and Secretion," embodying the views of Lagrange modified by further research, in the three following propositions:—

1. The difference in composition between arterial and venous blood consists chiefly in this, that the former contains an additional quantity of oxygen, and the latter of carbonic acid, chemically united with it; the affinity between the blood and the oxygen being more powerful than between the blood and carbonic acid.

2. The oxygen gas of the respired air, pervading the walls of the pulmonary vessels, displaces by virtue of its superior affinity an equal bulk of carbonic acid gas, and thus converts venous into arterial blood.

3. In the course of the circulation, the oxygen thus absorbed gradually attracts carbon from the proximate principles of the blood, and uniting with it produces heat, and by thus also forming carbonic acid converts the blood from arterial into venous.

The second part of the essay is devoted to the subject of animal heat, and Sir Benjamin Brodie's experiments are referred to and commented upon.

The author thinks that in this essay, written in 1823, he may be allowed to have anticipated by many years some of the views of Dumas and Liebig with regard to the changes in the blood through respiration.

Soon after taking the degree of M.D. at Edinburgh, Dr. Williams went to Paris, and there became acquainted with the physiologist Majendie, and saw many very eminent men of science, as La Place, Vauquelin, Ampère, Humboldt, Cuvier, Arago, Gay-Lussac, &c., and at the commencement of the Medical Session determined to make the Hospital of La Charité and the Clinique of Laennec the chief field of his work. An interesting description is given of Laennec, his slim frame, his weakness, coupled, however, with quickness of perception and intelligence, which enabled him to discover a new system and art for the elucidation of disease. Dr. Williams remarks that his teaching was little valued by the French students, and that his class chiefly consisted of foreigners and but a sprinkling of his own countrymen, these latter being more attracted by the impetuous Broussais, who captivated them by his grand ideas and sweeping hypotheses without troubling them with dry details or the results of careful observations. Dr. Williams also speaks most warmly of the knowledge of pathological anatomy which he obtained from Audral, and remarks that in eight months he learnt more of the subject than he could have done in eight years in the hospitals of his own country. Chomel and Louis were also his instructors, and there can be no doubt that his studies in Paris at this time had a great influence on

his future career, his mind being well fitted to make the best use of all the opportunities he then possessed.

Dr. Williams, speaking of Laennec, remarks that "the chief discoveries of auscultation and its large development were undoubtedly his, and have placed him in the foremost rank among the benefactors of mankind." To these, as well as to his personal teaching, says Dr. Williams, "I owe not only some of the most valuable knowledge that I have ever acquired, but also the opening up of new avenues of knowledge which will be inexhaustible to the end of time. It was the new idea of bringing another sense—the sense of hearing—to aid us in the investigation of the organs in health and disease, and through studying its indications, learning as it were language which would tell us of their changes of condition or motion, that gave vastness to the discoveries of Laennec, and would render them fruitful far beyond his own share in them."

After his return from Paris, Dr. Williams went first to Madeira with a patient, and subsequently for a few months to Switzerland with Lord and Lady Minto, as travelling physician.

After his return from this last journey, Dr. Williams' career as a London physician may be said to have commenced.

He took a house in Half Moon Street, Piccadilly, and devoted much time to preparing a work on Auscultation, chiefly of the lungs, and wrote several articles in the 'Cyclopædia of Practical Medicine,' and afterwards in the 'Library of Medicine;' in the latter work, most of the articles relating to diseases of the respiratory organs were committed to his charge. In his work on 'Auscultation,' his object was to bring in the laws of acoustics in order to explain the various marked phenomena, such as the fine crepitation in pneumonia and metallic tinkling signs which had been discovered, but in no way explained, by Laennec. In his article, "Coryza," in the 'Cyclopædia of Medicine,' the so-called *dry* method of cure was proposed.

Dr. Williams made some observations on slow combustion in 1823, and subsequently gave an evening lecture at the Royal Institution, and a paper was read at the Royal Society; Dr. Williams had always an idea, even when he wrote his Memoirs, that the subject had been shelved by scientific men: he thought it might have an important bearing on spontaneous combustion occurring in coal stores, hay-ricks, &c. The existence of slow forms of combustion, however, is quite recognised and appreciated by chemists and physicists, and the slow combustion of phosphorus must have been known since the discovery of that element.

In 1830 Dr. Williams married a maternal cousin, Miss Jenkins, and from 1828 to 1835 he was engaged in the investigation of the causes of the sounds of the heart, an investigation which was originated in conjunction with Dr. Hope, but unfortunately some

differences arose between him and his fellow-worker, a misunderstanding which afterwards became the source of much discomfort.

In 1835 Dr. Williams was elected a Fellow of the Royal Society, but, as will be shown later on, he disapproved of the existing constitution of that Society, preferring a more popular basis of membership, such as was to be found in the then young British Association.

On the resignation by Dr. Chambers of the Physicianship of St. George's Hospital, Dr. Williams offered himself as a candidate for that appointment, in opposition to Dr. Hope, but he soon retired from the candidature, finding that his chances would hardly justify a continuance of the contest.

In 1839 the Chair of Medicine at University College, with the Physicianship to the Hospital, became vacant by the retirement of Dr. Elliotson, and Dr. Williams secured the post for himself, and had thus an opportunity made for him where he might utilise his acquirements; he gave the Introductory Address at the opening of the Session, which was well received, as the writer of the present notice can testify, he having been one of the audience.

At University College and Hospital his colleagues were Drs. Sharpey, Anthony Todd Thomson, D. P. Davies, Dr. (afterwards Sir Robert) Carswell, Messrs. Liston, Samuel Cooper, and Richard Quain.

In 1840 Dr. Williams was nominated to the Fellowship of the Royal College of Physicians; he became a Fellow, but says he had some doubt as to accepting the honour, being opposed to the method of election of Fellows and to the constitution of the College altogether, and although great alterations were afterwards made by the passing of the Medical Act of 1858, still he always was more or less antagonistic to that body: a fact to which subsequent allusion will be made.

About this time Dr. Williams' attention was drawn to the preparation of his 'Principles of Medicine,' which appeared in 1843 and met with a hearty reception by the Profession.

In 1846 the Pathological Society was founded, and Dr. Williams was made the first President.

Dr. Williams soon became more and more engaged in private practice, and in 1848 resigned his appointment at University College and Hospital and removed to Upper Brook Street, where he remained till the end of his medical career, having been previously ten years in Half Moon Street and eleven years in Holles Street. In the 'London Journal of Medicine,' 1849 (a publication which only existed a very few years), Dr. Williams published a paper on the "Value of Cod-liver Oil in the Treatment of Pulmonary Consumption," and the conclusion arrived at was thus summarised:—

"I prescribed the oil in above 400 cases of tuberculous disease of the lungs in different stages, and of 243 of these have notes. Out of

this number the oil disagreed and was discontinued in only nine instances. In nineteen cases it appeared to do no good, while in the large proportion of 206 out of 234 its use was followed by marked and unequivocal improvement, varying in degree in different cases from a temporary retardation of the progress of the disease and mitigation of distressing symptoms up to a more or less complete restoration to apparent health."

Dr. Williams was by no means the first physician to prescribe cod-liver oil; Dr. Bardsley, of Manchester, had used it for many years, and Dr. Darling and Dr. Hughes Bennett had also anticipated him in the use of this most valuable therapeutic agent.

In 1852 Dr. Williams was telegraphed for to Walmer to see the great Duke of Wellington, but he arrived too late; nevertheless, he attended the State Funeral as a medical attendant of the Duke.

In 1858 Dr. Williams was elected President of the New Sydenham Society, which was established for the purpose of translating foreign works on medical subjects and the re-publication of important scattered papers.

In 1862 Dr. Williams delivered the Lumleian Lectures at the Royal College of Physicians, having previously been Gulstonian Lecturer in 1841.

We pass over the remaining years of Dr. Williams' career as a physician, as the increasing calls of a large practice left him little time for other work of more general interest, only briefly referring to his action for libel against the late Duke and Duchess of Somerset, which ended by his receiving an ample apology, completely clearing his professional character from the aspersions cast upon it in a period of excitement and distress at the loss of her son by the Duchess; and to the publication in 1871 of a work on Pulmonary Consumption, in conjunction with his son, Dr. C. Theodore Williams; and, lastly, his election as President of the Medical and Chirurgical Society, in 1873, and his appointment as Physician Extraordinary to the Queen, in 1874.

In the next year he retired from practice, and made his home at Cannes for the remainder of his life, dying there of pneumonia on March 24th, 1889. After his retirement, among other occupations of a naturally active mind, he spent some time in examining Sun-spots, especially in relation to weather changes, and made many summer visits to England.

This brief account of the life of Dr. Charles J. B. Williams would be very incomplete without the addition of some comments on the man and his work, suggested by the perusal of the Memoirs of his Life. One cannot help seeing that he was a man of unusual ability and of great confidence as to his powers—that he was industrious and persevering, and took full advantage of the opportunities

which were presented to him, of which characteristic a strong instance is supplied by his bringing at once into practical application in this country the powerful aid to diagnosis in diseases of the chest afforded by the stethoscope, whose value he had full opportunity of appreciating during his student career at Paris, as a pupil of Laennec. The frankness of his character is well shown in his treatment of different misunderstandings with professional brethren and others in the pages of his autobiography. He lays as much stress on what makes against himself as on the favourable points, so that where the impartial reader may hold that he was in the wrong he almost disarms hostile criticism at the moment when he arouses it. This frankness may very likely have been the outcome of deep religious conscientiousness, for the whole book, and especially the final chapter, gives evidence of his character in this respect. He knew how to do justice to others as well as to himself.

The reforming instinct of Dr. Williams found material to work upon in the constitution and bye-laws of the Royal Society and the Royal College of Physicians. With respect to the Royal Society, he endeavoured to introduce extensive changes by removing the limit put on the number of Fellows elected annually, and laid much stress on his conviction that, whereas the Royal Society was founded for the promotion of natural science, it had become a kind of club for the segregation of the *crême de la crême* among scientific men from the general mass. But, though a special committee was appointed to consider his proposals for reform, they reported in favour of the *status quo*.

The changes which he proposed in the case of the College of Physicians were of a very sweeping character, as he was eager that all physicians of good repute should be able, without difficulty or delay, to obtain the Fellowship as the full completion of the membership. He wanted the College to take a more commanding position—to become a national institution, fairly entitled to stand at the head of the Medical Corporations of the country and to acquire the authority and influence in the State that should properly belong to it; and he considered that, in order to attain this position, it was necessary for the College to include within its roll of membership many who did not belong to it, though fully qualified, because the privileges of mere membership were small, especially in the case of provincial physicians. But in this case, also, Dr. Williams found no adequate support for his views among the governing body of the institution.

His references to his family exhibit throughout many signs of a very affectionate nature, and one is glad to think that during his later years of ease and retirement such a man had fuller opportunities for enjoying domestic family life than among the bustle and engagements of a successful physician's mid-career.

The man was what his autobiography has shown. What was his work? His chief claim on the memory of his fellow-labourers and the public is that he was prominent among those who helped to reduce the principles of medicine to a properly co-ordinated scientific system, and who laid the foundations of modern pathology. It is pleasant to think that this high claim was fully recognised in his own lifetime, as is shown by his appointment to the different posts of honour already mentioned.

A. B. G.

By the death of Mr. Newall, which occurred in April, 1889, the Society has lost a Fellow whose labours and interest in the progress of instrumental astronomy have secured a notable progress, which has been utilized throughout the whole of the civilized world, his activity in this direction being in direct continuation of that of others to whom we owe in a large measure the renaissance of the optical art in England.

One of the first telescopes made by Cooke, of York, in his early days was one of large aperture constructed for Mr. Hugh Lee Pattinson, F.R.S., F.R.A.S., and this was almost the first commission for a large object glass he had received. It was in the satisfactory completion of this telescope that Cooke gave that sure promise of a combined optical and engineering skill which his whole life was destined to fulfil.

Mr. Newall, in 1849, married a daughter of Mr. Pattinson, and, possessing the same interest in astronomical pursuits as Mr. Pattinson himself, frequently discussed with him the possibility of successfully constructing and mounting an object glass of a size much larger than any other in existence. When he saw in the Exhibition of 1862 the two large discs of crown and flint glass, manufactured by the Messrs. Chance, of Birmingham, he determined to acquire them, and try an experiment. The diameter of these discs was about 26 inches; the largest existing refractor at that time in actual operation was the 16-inch at Pulkowa, and Mr. Newall determined to see whether or no it was possible to advance at one bound from an aperture of 16 inches to one of 25. This experiment was not a simple one, for the idea at that time was that even if a perfect object glass of such dimensions could be turned out by the optician's skill, yet that its flexure would render it practically useless for all fine astronomical purposes.

Our great optician, Cooke, was just as anxious to make this experiment as Mr. Newall himself, and he threw himself into the work with vigour. Ultimately the telescope was finished: Cooke had never made an object glass of which the definition was more perfect, while the admirable engineering skill displayed in the mounting and the ease

and simplicity of its motions and working had never been surpassed. Mr. Newall had originally intended to erect this enormous telescope in some climate more favourable for astronomical investigations than our own. Madeira, Egypt, and Malta were thought of, but it was first of all necessary to erect it not far from York in order that various experiments might be gone through without loss of time. It was, therefore, erected at Ferndene, Mr. Newall's residence at Gateshead, and after its completion, circumstances having arisen which prevented its transference abroad, it remained there until Mr. Newall's death.

By Mr. Newall's generosity it was practically at the disposal of any-one who had any special research to make for which a large aperture was indispensable. The writer of this notice would here acknowledge the many opportunities which were afforded him of making such observations, and also for the graceful hospitality with which these opportunities were accompanied.

The success of this experiment at once changed the aspect of the optical art in all countries. The 26-inch at Washington, the 27-inch at Vienna, and the 36-inch in California are the direct descendants of the 25-inch at Gatestead.

Before his death, Mr. Newall was anxious that the telescope should be removed to a more favourable locality, and it was offered by him to and accepted by the University of Cambridge. It is to be hoped that the scientific responsibilities which the University accepts with the instrument will be amply fulfilled.

It may be well understood that the practical sagacity, unswerving purpose, and scientific habit of mind which had led to the conception and final carrying out of such an experiment as this, in alliance with the elder Cooke, would prove fruitful in other fields. Mr. Newall was not only a successful manufacturer, but he may be regarded as one of the chief founders of one of the most important of our modern national industries. In that part of his business which had to deal with wire rope, he found an unmechanical method of working; he left one which is simply perfect, and, as a result, wire ropes of his construction are now found all over the world: the double process of making the strands and then combining them being entirely avoided, while the wires retain all their original strength, as they remain untwisted. He was among the first, if not the first, to see and subsequently demonstrate that the whole question of submarine telegraphy could only be settled by encasing the conducting wires with ropes similar to those he was constructing for other purposes. This, of course, necessitated the use of an insulating material, and Mr. Newall was among the first in this country to study the properties of gutta-percha, insulating the conducting wires by the material, and then surrounding and encasing them with strong wire rope.

The first successful cable laid from Dover to Cape Grisnez, in 1851,

was manufactured by Mr. Newall. After this successful experiment, of course greater lengths were tried. Ships were specially constructed for cable laying, and arrangements adopted for securing the greatest facilities in paying out.

Mr. Newall's sagacity was again shown, and he at once invented methods which have never been improved upon, and which are now universally adopted. In the tank in which the cables were coiled, a cone occupied the centre, and effectually prevented kinks in the paying out, while a "drum brake" was inserted in the paying out apparatus to prevent all undue strains. It has been well said:— "To have established a new industry, to have taken an active part in securing a triumph of applied science which will modify the history of the world, and to have led the way in the development of the refracting telescope is a record of achievement to which few attain, but which does bare justice to the life-work of Robert Stirling Newall."

Mr. Newall was born in 1812. He was D.C.L. of Durham University, and was elected a Fellow of this Society in 1875.

J. N. L.

John Percy, M.D., who died on the 19th of June last, was born at Nottingham, on the 23rd of March, 1817. At an early age he entered the Medical School of the University of Edinburgh, where at twenty-one he took the degree of M.D. He then studied in Paris, making the acquaintance of the leading French chemists of the time, who doubtless directed his mind towards the line of work to which his life was mainly devoted. He practised medicine at Birmingham for a few years, where, coming in contact with directors and managers of works, he was led to take special interest in metallurgy. It is, perhaps, worth remembering that the connexion between therapeutics and metallurgy has been traditional, and that the critical period of both was the middle of the 16th century, when Paracelsus attempted to introduce order into the science of medicine and Georgius Agricola strove to establish the art of metallurgy on a sound basis.

Dr. Percy's first paper was entitled a "Notice of a New Hydrated Phosphate of Lime," and his second, dealing with the "Management of Monkeys in Confinement," was printed in 1844. It was followed by other papers on medical subjects, but such work soon gave place to the systematic study of metallurgy, in which he might fairly say with an old writer: "An indefatigable labour, the closest inspection, and hands that were not afraid of the blackness of charcoal" had been his "chief masters." There was, indeed, little else than his own patient research to guide him, for the literature of metallurgy up to the time he wrote was sparse in the extreme, as may be gathered from the fact that when Cramer, himself a doctor of medicine, pub-

lished in 1754 his "list of the chief English authors (about thirty in all) who have treated of minerals and metals," none had written a treatise of metallurgy worthy of the name, though there were many detached monographs of value and a few papers in the 'Philosophical Transactions of the Royal Society.' In the period which elapsed, nearly a century, between the publication of Cramer's book and the time when Dr. Percy accepted the chair of metallurgy in the Royal School of Mines and began to teach, the most noteworthy contributions to metallurgical literature were Bishop Watson's Essays. These appeared in 1782, and are fragmentary, but, as Dr. Percy said, "are elegantly and lucidly composed, and I never take them up but with increasing pleasure." In 1861 Dr. Percy published the first volume of his treatise on 'Metallurgy,' which he dedicated "with sincere respect and affectionate regard" to Faraday. This work, which he calls the "task of his life," developed into a series of volumes containing 3,500 octavo pages. It is on this treatise that his reputation mainly rests, and we cannot doubt but that it will be enduring. The writings of Pliny in the 1st century, of Geber in the 8th, and of Agricola in the 16th, may still be read with profit side by side with the modern work of Karsten, Gay-Lussac, Berzelius, Le Play, Plattner, Deville, and Holley, and it is with these metallurgists that Percy takes his place. His writings differ in many ways from those of his predecessors in any country. He was forcibly impressed with the fact that metallurgical problems demand for their successful investigation the exercise of the highest analytical skill, and involve considerations worthy of those who delight in transcendental inquiries. He effectively quotes Réaumur's remark, "l'utile bien considéré a toujours du curieux, et il est rare que le curieux bien suivi ne mène pas à l'utile."

The distinctive character of his metallurgical treatises arises from the care with which he examined the relations of the metals to other elements and to each other. While his predecessors unhesitatingly accepted the statements of earlier writers or showed a tendency to deduce from analogy what these relations would be, he made the properties of metallic compounds the subject of careful experiment and embodied the results in his books, which form a record of great value, and one that teems with suggestions for future investigators. The excellence of the chemical portions of his books gives them great value as works of reference quite apart from the accurate and elaborate descriptions they contain of typical metallurgical processes. These were in all cases prepared by the best men he could find, usually his own students, who were actually engaged in conducting the operations they describe. Such aid was always fully acknowledged. One remarkable feature of these books is that almost every woodcut may be considered to be an accurate though small mechanical drawing, and it is

only measurable drawings of this kind which are really useful in practice. In criticising his writings it may perhaps be said that his dread of mere empiricism and his intolerance of inaccuracy often led him to magnify points which now seem to be somewhat trivial, and he sometimes withholds the expression of his own opinion when the reader has fairly a right to expect his guidance and would be grateful for the support of his authority.

Of all his work that relating to iron and steel is perhaps the most important, and it began early in his metallurgical career with an elaborate piece of analytical work. The first International Exhibition, of 1851, contained a very extensive and highly interesting series of British iron ores, collected with great labour and at considerable expense by Dr. Percy's friend, Mr. H. S. Bakewell, of Dudley, and afterwards presented by him to the Geological Museum in Jermyn-street. Mr. Bakewell placed at Dr. Percy's disposal the sum of £500 towards defraying the cost of analysing the more important of these ores. The work was completed by Dr. Percy with but slender aid from Government, and the results are embodied in his treatise on Iron and Steel, which was published in 1864. They have rightly been described as the first serious attempt at a survey of our national resources as regards ores of iron. With regard to the actual extraction of iron from its ores, his services were not less important. In 1855 the fact was established that pig iron from the blast furnace contains the greater part of the phosphorus originally present in the ore. Dr. Percy pointed out that phosphorus is not eliminated to a sensible degree in the Bessemer process, as it is in the old process of puddling; and he was of opinion that if the Bessemer process was to be "generally applicable in this country, it must be supplemented by the discovery of a method of producing pig iron sensibly free from phosphorus and sulphur with the fuel and ores which are now so extensively employed in our blast furnaces." The practical solution of the problem of eliminating phosphorus in the Bessemer converter, and the wide adoption of a process of truly national importance, are the outcome of Dr. Percy's teaching, for the problem was solved by three of his pupils. The delivery of his eloquent address in 1886, as President of the Iron and Steel Institute, fittingly ended the active portion of his labours with regard to these metals.

His most noteworthy addition to practical metallurgical processes was described in a paper written by him in 1848, and published in the same year in a journal called 'The Chemist.' A translation of this paper reached a distinguished Austrian, who introduced, at Joachimsthal, the process now known as that of von Patera, which depends on the solubility of chloride of silver in hyposulphite of soda. Early in 1884 this process was modified in America, by Mr. E. H. Russell, in order to render it applicable to ores poor in silver, which

also contain a considerable quantity of base metal. Those who have seen this process as now conducted in the Western States of America will appreciate the importance of Dr. Percy's original suggestion. His contributions to our knowledge of metallic alloys were of special value, and he discovered the alloy of copper and aluminium, now known as aluminium-bronze.

He concluded the introductory lecture which he delivered at the Royal School of Mines, more than a quarter of a century ago, by pointing out that "in proportion to the success with which the metallurgic art is practised in this country will the interests of the whole population, directly or indirectly, in no inconsiderable degree be promoted." The recognition of this fact appears to have steadily guided him, and his best services were always at the disposal of the Government and were freely used, as will be seen from the long list of Royal Commissions and departmental inquiries upon which he served. The first of these was the Committee appointed, in 1861, by the Secretary of State for War, to inquire into the "Application of Iron for defensive purposes." This continued its labours for four years, and was followed by a Special Committee appointed in 1867, to inquire into "Gibraltar" shields. He was also a member of the Royal Commission appointed in 1871, "to inquire into several matters relative to Coal in the United Kingdom," and of the Royal Commission of 1875, which investigated "the cause of the Spontaneous Combustion of Coal in Ships."

He resigned his chair at the Royal School of Mines in 1879, but he continued to hold the office of Lecturer on Metallurgy to the Advanced Class of Artillery Officers, at Woolwich. He was also Superintendent of the Ventilation, Warming, and Lighting of the Houses of Parliament. In both of these appointments he took great interest until his last illness. Dr. Percy collected with great care a series of metallurgical specimens to illustrate special points of interest relating to the manufacture and uses of metals. "From the study of it," he said, "I have myself derived much instruction. There are no specimens which, in my judgment, are more instructive than such as exhibit defects which have appeared either in the process of manufacture or in the use of metals." This collection was formed while Dr. Percy was at Birmingham and during the period of twenty-nine years he was Lecturer on Metallurgy at the Royal School of Mines. The specimens are of great interest and value, and, as in many cases they represent obsolete processes, no similar specimens could again be obtained. They are all minutely described by labels, many of which bear incidental references to Dr. Percy's Treatise on Metallurgy, to official reports, and to technical literature. It may safely be asserted that no existing metallurgical collection can compare with this in interest and importance. It is fortunate, therefore,

that it will not be dispersed, but, in accordance with the wishes of Dr. Percy's family, will be preserved in connexion with the School of Mines, where he taught so long, and exhibited to the public at South Kensington, as the "Percy Collection."

The most cursory examination of his writings will serve to show the rigid precision with which he wrote. Sometimes when his sympathy or indignation was aroused he would adopt a more florid though not less effective style, of which it would be difficult to find more characteristic examples than the two extracts here placed in conjunction, both relating to the exhaustion of our national supply of coal, and both being exponents of his patriotic wishes for the welfare of his country. Speaking of a well-known coal-field, he says, "This magnificent bed of coal has been most barbarously treated. The pits have generally been worked by contractors under the superintendence of viewers, called ground bailiffs. In consequence of the rapacity and rascality of many of the former, and of the ignorance, inattention, and fraudulent connivance of many of the latter, an enormous amount of coal has been irremediably lost to the nation." After an interval of ten years he said, in concluding his Presidential Address at the Iron and Steel Institute, "There is a question which must often occur to us, namely, what will Great Britain be when our vast reservoir of material force, coal, is exhausted . . . . The time must come when, in consequence of that exhaustion, Great Britain will cease to be a great manufacturing nation, . . . but, however mournful and unwelcome this proposition may be, we have the satisfaction of knowing that we are now laying the foundation of prosperous and mighty kingdoms in various parts of the world which we hope will be the strongholds of virtue, of order, and of freedom . . . . The glory of old England may, after all, not depart; on the sites of the soot-stained Birminghams and Manchesters new and splendid cities may arise where the merchant princes, of Anglo-Saxon descent, from the remotest parts of the globe shall rejoice to dwell and end their days in peace."

Dr. Percy's public utterances were but few, and the above formed a part of the last of them. He led a retired life and was hardly ever seen at Scientific Societies, though he was frequently at the Athenæum and was well-known at the Garrick Club.

He married in June, 1839, Miss Grace Mary Piercy, the only daughter of Mr. J. E. Piercy, of Warley Hall, near Birmingham. Those who knew him best feel that the loss of his wife, in 1880, greatly changed him.

Official recognition of his admirable labours there is none to record, but many Scientific Societies and Institutions conferred on him their membership. He was elected a Fellow of the Royal Society in 1847, and served on its Council in 1857-59; he was awarded the Millar Prize

of the Institution of Civil Engineers, of which body he was one of the few Honorary Members. He received the Bessemer Gold Medal of the Iron and Steel Institute in 1877, and, in 1889, the Prince of Wales, on the recommendation of the Council, awarded him the Albert Medal of the Society of Arts. The notification of the honour reached him on his death-bed, and he received it with the characteristic remark, almost his last, "My work is done."

W. C. R-A.

Admiral Sir Robert Spencer Robinson, K.C.B., was the youngest son of the late Venerable Sir John Robinson, Archdeacon of Armagh. Born in January, 1809, he entered the Navy 6th December, 1821, served in the boats of the "Sybille" in an affair with pirates in 1826, and became a Lieutenant in 1830. He attained the rank of Commander in 1838, and that of Captain in November, 1840. After commanding in the latter rank H.M.S. "Arrogant," "Colossus," and "Royal George" on foreign service, he was Captain of the Steam Reserves at Plymouth and Portsmouth for four years, until he became a Rear-Admiral, in 1860. He was a Commissioner to enquire into the management of the Royal Dockyards in the same year, and was appointed Controller of the Navy on 7th February, 1861, an office he held until 1871. The latter three years of this period he was also a Lord of the Admiralty. He retired in 1871, and died on the 27th July, 1889. He was the author of a treatise on the Marine Steam Engine. Sir Spencer was at the head of the constructive department of the Admiralty during a period of momentous change, when iron was being introduced in place of wood; the powers of the steam engine were being rapidly developed, and armour-plating was becoming a necessary protection to the battle ship. Having already profited by a scientific training, superior to that which in his day was common among naval officers, in one of the intervals of his naval employment by the Admiralty, he found an opportunity of entering the marine engine works of the late Mr. Robert Napier, on the Clyde, and there practically acquired the art of using engineering tools and of conducting factory work—acquirements which proved invaluable to him afterwards during his superintendence of the Steam Reserves, and subsequently of H.M. Dockyards. His service at the Admiralty, as Controller of the Navy, was distinguished by a combination of ability and devotion to duty well adapted to accomplish the solution of the great naval problems which it was his lot to grapple with, and indeed to solve. It was due to his influence, and largely to his initiative, that the British Navy became possessed of a fleet of iron-built armoured ships long before the necessity for giving up wood-built ships of that class was realised in other countries. He similarly anticipated foreign navies by the early introduction of the

compound marine engine; and throughout his period of service the British Navy led the way in the development both of armour and of ordnance. He was an accomplished linguist, and possessed great literary ability and aptitude.

W. J. J. W.

Charles Spence Bate was born at Trenick, near Truro, on the 16th of March, 1818. He was the elder son of Mr. Charles Bate, who practised as a dentist at Plymouth. He received his education at the Truro Grammar School, and afterwards was for two years in the surgery of Mr. Blewett. He then gave himself to the study of dentistry with his father, and on becoming qualified, commenced practice in Swansea, in 1841. Here he acquired a good practice, and by association with the scientific men of the place his taste for those branches of natural history with which his name was afterwards so closely associated was fostered and developed. After a residence of ten years in Swansea, Mr. Bate returned to Plymouth, where he succeeded to the dental practice of his father. In his profession he stood very high, enjoying a great repute for skill both in its purely surgical and mechanical branches. He contributed numerous papers on dental subjects to the 'Lancet,' the 'Medical Times and Gazette,' the 'British Journal of Dental Science,' and the 'Transactions of the Odontological Society.' He was President of the British Dental Association in 1883, of the Odontological Society of Great Britain in 1885, and of Section XII of the International Medical Congress held in London in 1881. As a naturalist Mr. Bate's favourite study was the Crustacea, and in the development and morphology of this group he did most of the work by which his name will long be remembered. The work which first brought his name prominently before the scientific world, was the 'Natural History of the British Sessile-Eyed Crustacea,' in the production of which he was associated with Professor Westwood. The difficulties of a work like this—embracing as it did so large a field in which but little had previously been done—were very great, and it cannot be wondered at that some of its conclusions and observations have already been superseded by others made under more favourable circumstances and with all the advantages of added experience and increased facilities of research. Still, however, this work remains the standard authority on its branch of Carcinology. Mr. Bate's most recent work was his report on the *Crustacea Macrura* of the "Challenger" Expedition. This laborious and comprehensive work occupies two large volumes of the Official Report of the Expedition, and is illustrated by 150 lithographic plates, almost all of them drawn by Mr. Bate. These are admirably done and of themselves form an enduring monument to the conscientious industry and

scientific acumen of the author, while the descriptive part of the work is marked by that thorough familiarity with the subject, which characterised his previous great work on the Sessile-Eyed Crustacea.

Mr. Bate contributed to the 'Proceedings of the Royal Society' papers "On the Development of *Carcinus Mœnas*" (1856–57), and "On the Development of the Crustacean Embryo and the Variations of Form exhibited in the Larvæ of 38 Genera of Podophthalmia" (1876). In the 'Philosophical Transactions,' 1858, appeared a memoir, "On the Development of Decapod Crustacea." These papers well show the interest which he took in the remarkable transformations undergone by the young of Crustacea, and the important share which he had in the elucidation of them. To the 'Proceedings of the Zoological Society,'—of which Society he was elected a Corresponding Member in 1865—Mr. Spence Bate contributed several papers descriptive of new species of Crustacea.

Mr. Bate was one of the founders of the Devonshire Association for the Advancement of Science, Literature, and Art, was its first Senior General Secretary in 1862, and its President in 1863. In this Society he always took the keenest interest, and he contributed to its Proceedings many papers, chiefly connected with local archæology. He was a most active member of the Plymouth Institution, was twice President of that body, and delivered before it many lectures on biological and archæological subjects. He was also deeply interested in the Fine Art Society of Plymouth, and being a good amateur artist in water-colours, he contributed frequently to the exhibitions held by that Society.

About two years before his death Mr. Bate partially retired from his dental practice, having previously purchased a country residence, —"the Rock," at South Brent—where he died after a brief illness on Monday, the 29th of July, 1889, aged 71 years. He was twice married. His first wife was Miss Emily Amelia Hele, of Wellad Lake, near Ashburton. His second wife survives him. He also leaves two sons of the first marriage,—Captain C. McGuire Bate, of the Royal Engineers, and Dr. Hele Bate, of London; and a daughter, Miss Emily Harriet Bate.

G. S. B.

John Frederic Bateman, a son of Mr. John Bateman, of Ockbrook, Derbyshire, was born the 30th of May, 1810. His mother was the daughter of the Rev. Benjamin La Trobe, an eminent Moravian minister in Yorkshire, in compliment to whom his grandson, late in life, assumed the name of John Frederic La Trobe Bateman.

At fifteen years of age he was apprenticed to a surveyor and mining engineer of considerable practice at Oldham, and made such progress that he was able to commence business on his own account, as an

engineer, at Manchester, in 1833. Here he had the good fortune to make the acquaintance of the eminent engineer, Mr. Fairbairn (afterwards Sir William Fairbairn, Bart., F.R.S.), who quickly saw the young man's merits, and formed a warm friendship with him, which resulted some years later in the marriage of Mr. Bateman with Mr. Fairbairn's daughter Anne.

In 1835 Mr. Fairbairn was applied to by the millowners on the River Bann, in County Down, Ireland, to examine the locality, and to report on the best means of improving the water power, which was in a very unsatisfactory state. Knowing that his young *protégé* had paid attention to water questions, he thought this would be a good means of bringing him forward, and he accordingly undertook the commission, associating Mr. Bateman with him in the work. In January, 1836, a report was published, signed by Mr. Fairbairn and Mr. Bateman jointly, giving full scientific calculations as to the hydraulic elements of the case, and recommending the construction of large reservoirs, and other works. An Act of Parliament was at once obtained, and in the course of the three following years the works were carried into execution, the designing and construction of the whole being entrusted, at Mr. Fairbairn's request, entirely to Mr. Bateman.

This work established Mr. Bateman's reputation as a hydraulic engineer, and led a few years later to his employment on a much larger and more important work, namely, the provision of an entirely new water supply, on the largest scale, for the city of Manchester. The town had previously been supplied by a private Company from sources near the town, but about 1844, when public attention became prominently directed to the sanitary condition of large towns, the supply was found so defective, that it was evident some extensive improvements must be made, and in that year Mr. Bateman was requested to advise the Company generally on the matter.

In the course of his work on the Bann Reservoirs, and in some subsequent practice in the Lancashire district, he had paid great attention to the mode of supply by impounding the rainfall in hilly districts, and to the data necessary for determining the capabilities of the system. He had become acquainted with the country surrounding Manchester, and had formed a strong opinion that the best source of supply would be by an application of the system to the hilly district lying to the east of the town, and in June, 1846, he made a report strongly recommending this scheme. He said:—"Within ten or twelve miles of Manchester, and six or seven miles from the present Gorton Reservoir (then supplying the town), there is a tract of mountain land abounding with springs of the purest quality. The physical and geological features offer such peculiar facilities for the collection, storage, and supply of water for the use of the towns in the plains

below, that I am surprised they should have so long been overlooked. There is no other district within reasonable limits, nor any source from whence water may be obtained, which will bear comparison with it."

The project as thus designed was on a small scale, suited to a Company who could not contemplate a large expenditure; but even this was considered too bold, and the measure, after encountering violent Parliamentary opposition, was abandoned.

A year or two afterwards the Corporation of Manchester determined to take the supply into their own hands, and Mr. Bateman was called in again by them. This time, however, being encouraged by the larger views that prevailed, he laid out a much bolder scheme, going at once into the heart of the district known as the valley of Longdendale, to which his views had already been directed. His plan was adopted by the Corporation, and was sanctioned by Acts of Parliament in 1847 and 1848. The municipality having arranged to buy up the old Company, the new works were put in hand. At first only such portions were executed as were absolutely necessary, but they were enlarged from time to time, as the demand increased, and it was not till 1877 that Mr. Bateman could report that they were completed, and that the water-bearing capabilities of the district were fully realised.

The work thus created under Mr. Bateman's hand was certainly a magnificent specimen of engineering power and skill. It consisted chiefly of a series of large artificial impounding lakes, extending over seven miles of the valley. Their aggregate content was 735,000,000 cubic feet, and they furnished water for nearly a million of people, as well as a very large and ample water compensation to the millowners on the stream. The total expenditure was about £3,000,000.

In addition to the reservoirs, the works comprised a multitude of subsidiary works for conveying and distributing the water, and for the regulation of the streams. Among these were several novel hydraulic contrivances of a high order; one an arrangement for the automatic separation of the clear from the peaty water (the former being sent to the town, and the latter stored for compensation), and another a mechanical gauge basin, by which the quantity of compensation water could be actually measured at any moment with great accuracy, for the satisfaction of the millowners.

Mr. Bateman was always justly proud of this his first great work; and one of the latest of his life's occupations was to prepare a handsome volume,* putting fully on record its entire history, and giving an

* "History and Description of the Manchester Waterworks." By John Frederic La Trobe Bateman, F.R.SS. L. and E. London and Manchester, 1884.

excellent description, profusely illustrated with engravings, of the various engineering constructions it contained.

In 1852 Mr. Bateman was applied to by the Corporation of Glasgow to advise them in regard to their waterworks. Many schemes had been proposed, but Mr. Bateman recommended them to obtain a supply from one of the most celebrated of the Scotch lakes, Loch Katrine. Parliamentary sanction was obtained in the session of 1854–5, and the works were finished in 1859. The loch, lying 367 feet above the sea, forms a large reservoir for the catchment basin above it, in which the rainfall is very large. To fit the lake for supply purposes, its level was raised 4 feet, and arrangements were made so that it could be drawn down 7 feet in all, giving an available storage of 5600 millions of gallons. The water is conveyed to the town by a conduit twenty-six miles in length.

In 1855 he was requested by the British Association to prepare a general report on water supply, and, in pursuance of the request, he presented, at the meeting at Glasgow, in September of that year, a communication "On the present state of our knowledge on the Supply of Water to Towns. By John Frederic Bateman, F.G.S." It was a paper of some length and considerable merit. After stating the general nature of the problem, and giving a historical outline of previous measures, it enumerated the various sources from which towns could be supplied, and discussed their comparative merits, adding examples and statistical data in illustration.

In 1861, when the British Association held their meeting at Manchester, under the Presidency of Mr. Fairbairn, his son-in-law undertook the post of President of the Mechanical Section.

Mr. Bateman was elected a Fellow of the Royal Society on June 7, 1860. He served on the Council in the years 1865 and 1866.

At the end of 1869, the Viceroy of Egypt invited the Royal Society to send a representative to be present at the opening of the Suez Canal, and on the recommendation of the President, General Sir Edward Sabine, Mr. Bateman was selected for the duty. On his return he wrote to the President a long report of his visit, which was read to the Society on the 6th January, 1870, and was published in full in the 'Proceedings.' Mr. Bateman gave an interesting historical notice of the negotiations and proceedings which had been going on for many years on the subject, and had terminated in the successful completion of the enterprise. He added a general description of the Canal and of the ceremony of its opening, and he concluded with the following eulogium:—

"The Canal must be regarded as a great work, more from its relation to the national and commercial interests of the world than from its engineering features. In this light it is impossible to overestimate its importance. It will effect a total revolution in the mode

of conducting the great traffic between the East and the West, the beneficial effects of which, I believe, it is difficult to realise. It is in this sense that the undertaking must be regarded as a great one, and its accomplishment is due mainly to the rare courage and indomitable perseverance of M. Ferdinand de Lesseps, who well deserves the respect he has created and the praises which have been bestowed. . . . A channel of water communication has been opened between the East and the West which will never again be closed so long as mercantile property lasts or civilisation exists."

The waterworks which Mr. Bateman had constructed for Manchester and for Glasgow, although both of gigantic magnitude, did not satisfy his ambition. He saw the treasures of water which were annually wasted by floods in mountain districts, and he longed to originate some great schemes for utilising them. He said:—

"I could never see the wisdom of the view which would confine the supply of water to the towns and places which lay within any particular watershed. Where the water was most abundant it was generally the least wanted; and towns had grown up where it was often difficult to obtain this essential contribution to life and prosperity. To my mind, the idea of confining such places to their own watersheds, and preventing their going for what they wanted where there was enough and to spare, was absurd. As well might it be urged that the coal produced in the neighbourhood of Newcastle should all be consumed in the Valley of the Tyne, and none of it conveyed to London or the Valley of the Thames."

About 1869 an opportunity offered for exemplifying this principle. There had been a good deal of discussion as to the water supply of London, and Mr. Bateman designed a project for supplying the metropolis from a mountainous district in North Wales, where the rainfall was very large. He proposed to collect the water in artificial reservoirs, and to bring it to London by conduits above 180 miles long, delivering it at such a level as would give the supply entirely by gravitation. The plan was submitted to a Royal Commission, of which the Duke of Richmond was Chairman, and who reported in 1868. They expressed a high opinion of the plan, but decided that, for the present, at least, the metropolis did not require so expensive a measure.

Some years later, however, Mr. Bateman's ambition was gratified in another way. When, in 1877, he reported to the Corporation of Manchester that the waterworks were finished, he did not conceal from them that he had for some time been anxious about the future, inasmuch as the enormous growth of the town was fast outrunning the capacity, great as it was, of the Longdendale source of supply. He reverted to his former principle of resorting to mountain districts,

where large quantities of water were running to waste, and he found this eminently the case in the Lake District of Cumberland. The Corporation took up the idea, and in 1879 an Act was passed for obtaining a large supply from Thirlmere by a conduit 100 miles long. In this Mr. Bateman was associated with Mr. George Hill, of Manchester, by whom the works are now being carried out.

Mr. Bateman's knowledge of water supply involved several points of original scientific investigation. The chief one was on the subject of rainfall and its accompanying phenomena. When he undertook the work of the Bann Reservoirs, in 1835, he was surprised to find how little trustworthy information was available as to the two points of greatest importance, namely: (1) the rainfall upon the surface, and (2) the proportion of this that flowed off in the streams and rivers. With laudable zeal and industry he determined to remedy this difficulty for the future by establishing on the ground a regular system of observations on both points. After this time, in the course of his Manchester practice, he was engaged, more or less directly, on several other proposals or undertakings which had to do with water supply in the neighbourhood; and in every one of these he followed up his enquiry either by obtaining the best records available, or making new sets of observations entirely his own.

The knowledge gained in these enquiries emboldened him to read two papers before the Literary and Philosophical Society of Manchester, viz.:—

"Observations on the Relation which the fall of Rain bears to the Water flowing from the Ground" (read 6th February, 1844); and

"Report of the Committee for superintending the placing of Rain Gauges along the lines of the Rochdale, Ashton-under-Lyne, and Peak Forest Canals. With observations, &c." (read 18th March, 1845).

These early papers contained valuable facts, and showed considerable power of scientific reasoning upon them. But the subject was a favourite one with Mr. Bateman during his whole career. He seldom wrote a report on water undertakings without giving explanations thereon, and, as late as 1883, he wrote a special paper for the Victoria Institute, entitled "Meteorology and Rainfall."

He was a great advocate for the use of soft water; and one of his later works, in the Colne Valley near Watford, was remarkable for his successful application of the elegant chemical principle of the late Dr. Clark, for softening hard chalk water by lime; a great boon to the populations supplied.

In 1878, Mr. Bateman was elected President of the Institution of Civil Engineers, an office which he filled two years. In his opening address, delivered 15th January, 1878, in addition to the usual professional topics, he enlarged on the subject of engineering education, pointing out particularly the necessity for the cultivation of

science in those branches that bear on the operations of the profession.

It has only been possible here to mention a few of the works by which Mr. Bateman has been most widely known, but his practice was very large, and he occupied a high position in his profession.

Mr. Bateman died at his residence, Moor Park, Farnham, on the 10th of June, 1889.

W. P.

INDEX TO VOL. XLVI.

ERRATA.

Page 211, line 14 from bottom, *for* "1887" *read* "1877."
„ 212 „ 21 „ „ "phenylhydrazide" *read* "phenylhydrazone."
„ 449 „ 3 „ „ "with one exception the senior of our" *read* "one of our senior."

END OF FORTY-SIXTH VOLUME.

HARRISON AND SONS, PRINTERS IN ORDINARY TO HER MAJESTY, ST. MARTIN'S LANE.

Sold by Harrison and Sons, St. Martin's Lane.

PHILOSOPHICAL TRANSACTIONS.

Price £1 10*s*., with 23 Plates.

SERIES A. Containing Papers of a Mathematical or Physical Character.

VOL. CLXXIX (1888).

Price £2 17*s*. 6*d*., with 80 Plates.

SERIES B. Containing Papers of a Biological Character.

VOL. CLXXIX (1888).

Cloth cases for binding the above volumes can be obtained of Harrison and Sons, St. Martin's Lane. Price 1*s*. 6*d*.

The Papers in each Series can also be purchased separately, and are published by Trübner & Co., 57, Ludgate Hill. The following are now ready:—

SERIES A.

VOL. CLXXX (1889).

44. Some Observations on the Amount of Light reflected and transmitted by certain kinds of Glass. By Sir JOHN CONROY, Bart., M.A., Bedford Lecturer of Balliol College, and Millard Lecturer of Trinity College, Oxford. Price 2*s*. 6*d*.
45. On the Total Solar Eclipse of August 29, 1886. By Captain L. DARWIN, R.E., ARTHUR SCHUSTER, Ph.D., F.R.S., and E. WALTER MAUNDER. Price 4*s*. 6*d*.
46. Report of the Observations of the Total Solar Eclipse of August 29, 1886, made at the Island of Carriacou. By Rev. S. J. PERRY, S.J., F.R.S. Price 1*s*.
47. On the Determination of the Photometric Intensity of the Coronal Light during the Solar Eclipse of August 28–29, 1886. By Captain W. DE W. ABNEY, C.B., R.E., F.R.S., and T. E. THORPE, Ph.D., F.R.S. Price 1*s*. 6*d*.
48. Report of the Observations of the Total Solar Eclipse of August 29, 1886, made at Grenville, in the Island of Grenada. By H. H. TURNER, M.A., B.Sc., Fellow of Trinity College, Cambridge. Price 1*s*.
49. Revision of the Atomic Weight of Gold. By J. W. MALLET, F.R.S., Professor of Chemistry in the University of Virginia. Price 2*s*. 6*d*.
50. Magnetic and other Physical Properties of Iron at a High Temperature. By JOHN HOPKINSON, M.A., D.Sc., F.R.S. Price 4*s*. 6*d*.
51. The Diurnal Variation of Terrestrial Magnetism. By ARTHUR SCHUSTER, F.R.S., Professor of Physics in Owens College. With an Appendix by H. LAMB, F.R.S., Professor of Mathematics in Owens College. Price 2*s*. 6*d*.

SERIES B.

VOL. CLXXX (1889).

44. On the Electromotive Changes connected with the Beat of the Mammalian Heart, and of the Human Heart in particular. By AUGUSTUS D. WALLER, M.D. Price 2*s*.
45. On the Organisation of the Fossil Plants of the Coal-Measures. Part XVI. By WILLIAM CRAWFORD WILLIAMSON, LL.D., F.R.S., Professor of Botany in the Owens College, Manchester. Price 3*s*.
46. Researches on the Structure, Organization, and Classification of the Fossil Reptilia. VI. On the Anomodont Reptilia and their Allies. By H. G. SEELEY, F.R.S., Professor of Geography in King's College, London. Price 8*s*. 6*d*.
47. On some Variations of *Cardium Edule* apparently Correlated to the Conditions of Life. By WILLIAM BATESON, M.A., Fellow of St. John's College, Cambridge, and Balfour Student in the University. Price 6*s*.
48. On the Descending Degenerations which follow Lesions of the Gyrus Marginalis and Gyrus Fornicatus in Monkeys. By E. P. FRANCE. With an Introduction by Professor SCHÄFER, F.R.S. (From the Physiological Laboratory, University College, London.) Price 5*s*.

NOTICES TO FELLOWS OF THE ROYAL SOCIETY.

Government Grant for the Promotion of Scientific Research.—*The last day* for receiving Applications is February 28th.

A Monograph of the Horny Sponges, by Dr. R. von Lendenfeld, is now published (see above). The price to the public is £3; to Fellows of the Royal Society, £2. As the edition is limited to 500 copies, Fellows should order their copies at once of the Assistant Secretary.

The following are now offered *to Fellows only* at reduced prices :—
"Catalogue of the Scientific Books in the Library of the Royal Society"—

Part 1.	Transactions, Journals, &c.	4*s*. net.
Part 2.	General Catalogue	7*s*. „
	The two Parts	10*s*. „

"International Polar Expedition. Fort Rae Observations." 10*s*. 6*d*. net.
"Catalogue of Scientific Papers," in sets :—

	Net prices. £	*s*.	*d*.	
Vols. I—VI in Half-Morocco at	3	10	0	the set.
*Vol. I, Half-Morocco, and II—VI, Cloth. .	2	14	6	„
Vols. VII, VIII in Half-Morocco at . .	1	10	0	the two.
„ „ „ in Cloth	1	1	0	„

The Library is now open from 11 A.M. to 6 P.M. (Saturdays 1 P.M.).

Philosophical Transactions.—The Fellows are now permitted to give standing orders for the delivery of the volumes of 'Philosophical Transactions' to their addresses as published. The order must be in writing, and should be sent to the Assistant Secretary.

On application to Messrs. Harrison and Sons, 46, St. Martin's Lane, Fellows and others can have the separate numbers of the 'Philosophical Transactions' bound in cloth case for 2*s*. 6*d*., or can obtain cloth cases for binding, price 1*s*. 6*d*.

List of Fellows.—Fellows who desire to have the Annual List posted to them every year as soon as issued should send their names to the Assistant Secretary.

A printed post-card of the papers to be read at each meeting will be sent weekly to any Fellow upon application to Messrs. Harrison and Sons, 46, St. Martin's Lane, W.C.

* Vol. I in cloth is sold out.

HARRISON AND SONS, 45 & 46, ST. MARTIN'S LANE, W.C.

AND ALL BOOKSELLERS.